The Economic Costs of Mass Shootings: Evidence from U.S. Housing Markets

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Abstract

We study how mass shootings affect local housing markets through information-based belief updating. Housing-market conditions deteriorate following a mass shooting: buyer interest declines, homes take longer to sell, price concessions become more frequent, and listing prices fall. The decline in prices indicates that reduced willingness to pay on the demand side dominates any offsetting effects from temporary supply withdrawal. Housing-market responses scale with the objective severity of incidents, are amplified by public attention, and persist beyond the immediate aftermath. We find no evidence that these effects operate through contemporaneous mortgage credit contractions or population reallocation, consistent with belief-based rather than credit- or migration-driven channels.

Keywords: Mass shootings; Local housing markets; Market hotness; Demand and supply; Online search activity; Listing dynamics; Prices.

JEL Codes: R21, R31, K42

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1. Introduction

Mass shootings are among the most traumatic events communities experience, leaving lasting emotional, psychological, and social scars (North, Smith, and Spitznagel, 1994, 1997; DiScala and Sege, 2004; Lowe and Galea, 2015). Beyond the immediate human toll, such events alter perceptions of safety, disrupt daily routines, and erode trust in public spaces. Their sudden and violent nature, often amplified by intense media coverage, makes them especially salient local shocks. While national attention may fade quickly, the consequences for residents can persist, shaping decisions about where to live, work, and invest. These dynamics suggest that mass shootings may have enduring effects on local economic behavior and asset markets.

Consistent with this view, prior research shows that mass shootings impose substantial economic costs beyond human tragedy. They depress local economic activity, weaken consumer and business sentiment, and impair labor productivity through deteriorations in mental health, particularly when incidents receive heightened media attention (Brodeur and Yousaf, 2025). Employment declines in affected areas, with larger losses in sectors reliant on face-to-face interactions (Gayán-Navarro and Sanso-Navarro, 2025). Despite this evidence, comparatively little is known about how mass shootings affect the desirability of living in affected communities. We address this gap by studying how mass shootings influence local housing markets—the primary asset for most households and a key barometer of neighborhood desirability.

Our analysis is motivated by a hedonic framework in which local crime reduces neighborhood desirability by increasing perceived victimization risk and lowering the expected utility of residence, effects that capitalize into weaker housing demand and lower prices (Thaler, 1978; Gibbons, 2004). Consistent with this mechanism, properties in high-crime areas sell at systematic discounts (Lynch and Rasmussen, 2001). Gun violence further amplifies these effects by heightening perceptions of danger, weakening informal social control, and reducing local economic activity (Cook and Ludwig, 2006). Mass shootings—an extreme and highly salient form of gun violence—magnify fear and stigma well beyond baseline crime levels, often reinforced by extensive media coverage. Even after public attention subsides, declines in perceived safety and social cohesion may persist, prompting households to reassess residential location choices.

We examine whether mass shooting incidents measurably affect local housing-market conditions, and through which channels, using ZIP-code-level data from August 2017 to December 2023. We measure housing-market conditions using Realtor.com's Market Hotness Index, an equally weighted composite of a Demand Score and a Supply Score. The Demand Score captures buyer interest through unique listing-page views and related engagement metrics, while the Supply Score reflects market tightness through days on market, inventory, and pricing adjustments. Higher values indicate tighter, seller-leaning markets, while lower values reflect looser, buyer-leaning conditions. The index is normalized nationally, allowing direct comparison across local markets (Research – Market Hotness Index; Residential Real Estate Data – Data Dictionary).

Our baseline hypothesis is that mass shootings weaken local housing-market conditions. The mechanism follows standard demand–supply logic. A mass shooting raises perceived risk and stigma, reducing buyer engagement and willingness to pay. On the supply side, some sellers delay entry while others accept price concessions, increasing time on market. Whether prices fall depends on the relative strength of these forces: if demand contracts more than supply withdraws, prices decline; if supply withdraws sharply, price effects may be muted even as liquidity worsens. Our empirical design separately identifies demand-side, supply-side, and price responses, allowing us to assess which channel dominates and how housing valuations adjust.

To estimate causal effects, we exploit the precise timing and ZIP-code location of mass shootings and compare affected ZIP codes with observationally similar but unaffected ZIP codes drawn from the national universe. Because mass shootings are rare events, we construct a matched control group using nearest-neighbor propensity scores based on lagged housing-market conditions and a rich set of predetermined local characteristics, with matches constrained to the same year–month. We then estimate panel regressions with ZIP and year–month fixed effects. Identification comes from within-ZIP deviations around the incident relative to matched controls. Balance diagnostics and event-time placebo tests show no pre-treatment effects, supporting the parallel-trends assumption.

We find that local housing-market conditions weaken following a mass shooting. Affected ZIP codes experience a statistically and economically meaningful deterioration in market hotness relative to matched controls, sufficient to shift markets toward looser, less competitive conditions. Decomposing the response, we document a clear demand-side contraction: buyer interest declines, as reflected in both a deterioration in national demand rankings and a reduction in attention intensity relative to the national benchmark. On the supply side, markets experience slower transactions, fewer new listings, and a higher prevalence of price reductions, indicating that sellers respond both by delaying entry and by adjusting prices downward. Importantly, listing prices fall following a mass shooting, indicating that reduced willingness to pay on the demand side dominates any offsetting price pressure from temporary supply withdrawal.

We further examine heterogeneity and mechanisms. Housing-market responses scale with the objective severity of incidents and are significantly stronger when local public attention exceeds national benchmarks, consistent with an information-based salience channel. By contrast, we find little evidence that the effects operate through population reallocation or contemporaneous mortgage-credit contractions. Areas with tighter pre-existing credit conditions exhibit attenuated, rather than amplified, responses, a pattern inconsistent with lender pullbacks and instead suggestive of demand-side belief updating. Taken together, these findings reinforce an interpretation of mass shootings as information shocks that alter perceptions of safety and long-run residential desirability.

Our paper contributes to the household finance literature by showing that rare, highly salient violent shocks—specifically mass shootings—can durably affect household wealth through

housing assets. While prior work has documented the effects of low-probability shocks such as natural disasters (Gallagher and Hartley, 2017; Gao, Liu, and Shi, 2020; Bharath and Cho, 2022; Billings, Gallagher, and Ricketts, 2022; Jedwab, Johnson, and Koyama, 2022; Gallagher, Billings, and Ricketts, 2023; Begley et al., 2024; Collier and Ellis, 2024) and terrorism (Voigtländer and Voth, 2012; Besley and Mueller, 2012; Brodeur, 2018), this is the first study to examine the economic impact of mass shootings on housing markets. We show that these episodic, localized events generate significant and persistent deterioration in market activity and listing valuations, driven primarily by demand-side retrenchment rather than credit or migration channels. Treating mass shootings as salient information shocks, we provide evidence that belief-based responses—centered on perceived safety and stigma—can trigger durable asset repricing and liquidity loss, even in the absence of structural economic damage. Our findings challenge conventional models of housing valuation by highlighting the economic consequences of perception-driven shocks.

The remainder of the paper proceeds as follows. Section 2 describes the data and sample construction. Section 3 develops the identification strategy and matched-control design. Section 4 presents the baseline results and decomposes the response into demand, supply, and price channels. Section 5 concludes.

2. Data Description and Sample Selection

We assemble a ZIP-code–level panel to examine the effects of mass shootings on local housing market conditions. The primary housing market data come from Realtor.com and include monthly measures of active listings, median listing prices, and median days on market for all U.S. ZIP codes. The sample spans July 2016 to March 2025, allowing for a detailed analysis of both cross-sectional variation and temporal trends in local housing supply.

Market Hotness Index. To capture relative market competitiveness, we use Realtor.com's Market Hotness Index, which combines median days on market (a proxy for supply conditions) with the number of unique property views (a proxy for buyer demand) to produce a standardized Market hotness index at the ZIP-code level. Higher scores indicate faster-selling properties and greater buyer interest. The Hotness Index is available from August 2017 to January 2025, which determines the start of our main analysis period.

Mass shooting data. Incident data are drawn from the Gun Violence Archive (GVA), a nonprofit that compiles and verifies gun violence incidents from public sources. GVA defines a mass shooting as an event in which four or more individuals are shot (injured or killed), excluding the shooter, regardless of fatalities (Gun Violence Archive – General Methodology).

COVID-19 data. To control for pandemic-related disruptions, we use daily county-level COVID-19 cases and deaths from the New York Times. We remove erroneous negative entries—typically stemming from data revisions—and compute monthly averages.

Socio-economic and demographic controls. Annual county-level data from the County Health Rankings database include violent crime rates, injury death rates, social association rates, the percentage of African-American residents, median household income, and income inequality. Violent crime data are available through 2022; because our baseline regressions include a one-year lag of this variable (see Section 3), our sample ends in 2023. Additional economic indicators—population growth, per capita personal income, and GDP growth—are obtained from the Bureau of Economic Analysis, while annual unemployment rates come from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) program.

Housing returns. Monthly ZIP-code–level housing returns are constructed from the Zillow Housing Research Database.

We exclude ZIP-month observations with missing values and winsorize all continuous variables at the 1st and 99th percentiles to mitigate the influence of outliers. The final dataset comprises 44,760 ZIP-month observations from August 2017 to December 2023.

3. Identification Strategy

To estimate the localized impact of mass shootings on real estate markets, we exploit precise information on the timing and geographic location (ZIP-code level) of each event. Because such incidents are rare, conventional panel regressions may lack sufficient power to detect their effects. We address this by constructing a matched control sample from the universe of U.S. ZIP codes over an extended horizon, comparing affected ZIP codes with observationally similar but unaffected counterparts. This approach improves identification while mitigating bias from unobserved heterogeneity and broader market trends.

Matching procedure. We implement nearest-neighbor propensity score matching (Rosenbaum and Rubin, 1993). A logit model predicts treatment status using the one-, two-, and three-month lagged real estate market hotness index, along with controls for the COVID-19 period, violent crime rate, injury mortality rate, demographic and social characteristics (fraction African American, population growth, income inequality, median household income, per capita personal income, and number of social associations), economic conditions (unemployment rate and GDP growth), and housing market performance (housing return). These control variables are lagged one year, except for the housing return. The housing return is lagged one month. All variables are defined in Appendix A. Year–month fixed effects ensure treated ZIPs are matched to controls in the same month–year. Each treated ZIP is matched to its three nearest untreated neighbors based on predicted probabilities, with replacement allowed (Smith and Todd, 2005). The final sample includes 1,559 treated ZIP codes and 4,677 control ZIP codes.

Diagnostic tests. Figure 1 plots the standardized percentage bias for each covariate before and after matching (● unmatched, × matched), computed as in Rosenbaum and Rubin (1985). Bias declines sharply for nearly all covariates, approaching zero in most cases. Table 1 confirms this improvement: the "% reduction of |bias|" column shows near-complete elimination of imbalance

for most variables, t-tests indicate statistically insignificant mean differences, and variance ratios for continuous covariates are close to one.

Insert Figure 1 and Table 1

Pre-trend analysis. Figure 2 presents in-time placebo tests, shifting the treatment date backward by 5, 10, 15, and 20 months and re-estimating the model. Across all placebo periods, estimated effects are small and statistically indistinguishable from zero, providing no evidence of anticipatory behavior or spurious pre-trends. This absence of pre-treatment effects supports the parallel trends assumption and strengthens the causal interpretation of our baseline estimates.

These diagnostics indicate that our matching procedure achieves strong covariate balance and that estimated effects are unlikely to be driven by pre-existing trends, supporting the credibility of our causal interpretation.

Insert Figure 2

4. Results

This section quantifies the causal effect of mass shootings on local housing market competitiveness, measured by the real estate Market hotness index. Leveraging the matched sample from Section 3, which balances treated and control ZIP codes on pre-event characteristics, we isolate the market response to the shooting shock from contemporaneous local and aggregate factors. Our estimates reveal a sizable and statistically significant decline in market hotness following a mass shooting, indicating that such events materially reduce the attractiveness and competitiveness of the affected housing market. We begin by outlining our baseline regression specification (Section 4.1) and presenting the main treatment effects (Section 4.2). We then explore the mechanisms by examining demand, supply, and price channels (Sections 4.3–4.5), before turning to robustness and validation checks (Section 4.6).

4.1 Main Specification

We begin by introducing our baseline regression framework, which estimates the effect of mass shooting incidents on local housing-market conditions as measured by the Market Hotness Index. The specification controls for time-invariant location characteristics, common aggregate shocks, and a rich set of time-varying local economic and demographic factors that may influence housing-market dynamics. Our baseline model is a linear panel regression with multiple fixed effects. Detailed definitions of all variables and their data sources are provided in Appendix A.

Specifically, we estimate the following model:

$$\text{Market hotness index}_{z,t} = \beta_j \, Mass\ shooting_{z,t} + X'_{z,t-1}\,\gamma + \alpha_z + \delta_t + \varepsilon_{z,t} \quad (1),$$

where $\text{Mass Shooting}_{z,t}$ is an indicator equal to one if a mass shooting occurred in ZIP code zj months prior and zero otherwise. The vector $X_{z,t-1}$ includes local economic and demographic

controls. The control variables are lagged one year, except for the one month-lagged housing return. The $\alpha_z$denotes ZIP-code fixed effects, and $\delta_t$denotes month fixed effects. All reported standard errors are heteroskedasticity-robust and clustered at the ZIP-code level.

The dependent variable is the Market Hotness Index, a comprehensive measure of local housing-market conditions. Table 2 reports descriptive statistics for the full sample of 41,838 ZIP–month observations, providing an overview of both the dependent and explanatory variables used in the analysis. The mean Market Hotness Index is 48.71 (standard deviation = 22.09), with values ranging from 3.65 to 95.99, indicating substantial heterogeneity in housing-market activity across locations.

Our primary variable of interest is a mass-shooting indicator that equals one for ZIP code–month observations in which a mass shooting occurred and zero otherwise. As reported in Table 2, the mean of this indicator is 0.04, indicating that approximately 4% of ZIP code–month observations in the sample involve a mass shooting incident. We provide more detailed descriptive evidence on the nature of these events in Table 3, which summarizes incident characteristics for the subsample of ZIP codes that experienced at least one mass shooting.

Mass shootings in the sample display substantial heterogeneity in severity. On average, an incident results in 5.88 total casualties, with a median of 5.00 and a maximum of 23. Fatalities constitute a relatively small share of total casualties: the mean number of victims killed per event is 1.13, compared to an average of 4.72 individuals injured. This pattern highlights that most mass shootings involve multiple non-fatal injuries rather than exceptionally high fatality counts. Outcomes involving suspects are comparatively infrequent. The mean number of suspects killed or injured per incident is 0.06 and 0.05, respectively, while the average number of suspects arrested is 0.70. In slightly more than half of incidents (mean = 0.51), no suspect is identified or apprehended.

The distribution of incident characteristics suggests that, although most mass shootings involve relatively few fatalities, the injury toll can be considerable, potentially generating significant local social and psychological impacts. Moreover, the high fraction of cases without an identified suspect may prolong uncertainty and distress within affected communities, thereby exacerbating perceived neighborhood risk. The wide dispersion in casualty counts—from events barely exceeding the four-victim threshold to extreme outliers with more than 20 casualties—underscores heterogeneity in both the scale of violence and its likely economic and real estate repercussions.

Table 4 reports state-level summary statistics on the incidence, severity, and enforcement outcomes of mass shootings. Consistent with population size and urban density, high-population states account for the largest number of incidents. California records the highest number of mass shootings (191), followed by Texas (163), Pennsylvania (156), Louisiana (119), Florida (113), and Georgia (107). These patterns align with greater exposure in larger and more urbanized states.

Severity, however, varies substantially across states and is not mechanically related to incident frequency. Pennsylvania exhibits the highest average severity, with a mean of 8.68 total casualties per incident, driven primarily by a high number of injuries (7.15). Nevada also stands out, with an average of 7.70 casualties despite a relatively small number of incidents (13), underscoring that infrequent events can nonetheless be highly severe. The District of Columbia combines relatively frequent exposure (58 incidents) with elevated average severity (7.66 casualties), largely reflecting a high mean number of injuries (6.50).

The table further reveals pronounced cross-state heterogeneity in investigative and enforcement outcomes. The share of incidents with no identified suspect is particularly high in Delaware (83%), Michigan (66%), New Jersey (65%), and Ohio (62%), suggesting either investigative challenges or differences in incident characteristics. Arrest outcomes also vary widely: states such as Nevada (mean arrests of 1.40), South Carolina (1.21), Wisconsin (1.36), and Utah (3.00, driven by a small number of incidents) average more than one arrest per event, consistent with multi-suspect episodes. While the fraction of suspects killed or injured is generally low, it spikes in states with small samples and isolated high-severity incidents, such as Idaho and Utah.

Taken together, these descriptive patterns highlight substantial heterogeneity not only in the frequency of mass shootings, but also in their severity and resolution. This variation underscores that mass shootings differ meaningfully in their informational content across locations—an important consideration for understanding how such events shape local perceptions of safety and, ultimately, housing-market responses.

We next discuss each control variable included in the baseline regression model.

COVID-19 Period. The onset of COVID-19 generated unprecedented shocks to U.S. housing markets, reshaping real estate hotness through intertwined demand-, supply-, and risk-based channels. On the demand side, historically low mortgage rates—driven by aggressive monetary easing—combined with heightened preferences for larger living spaces, suburban or exurban locations, and remote work flexibility, expanded geographic choice sets and intensified buyer competition (Howard, Liebersohn, and Ozimek, 2023; Van Nieuwerburgh, 2023). Consistent with prior work documenting a "mortality risk premium" in mortgage rates—where lenders charged higher rates in MSAs with elevated COVID-19 mortality risks, even after controlling for creditworthiness and fundamentals—the pandemic also introduced a pricing risk shock, constraining affordability in high-risk areas and moderating demand (Gill, 2024). On the supply side, construction delays, labor shortages, and supply chain disruptions reduced new housing availability, while heightened uncertainty led some sellers to postpone listings, further tightening inventory (Zhao, 2020). These forces jointly produced a heterogeneous reallocation of housing demand: safer, lower-density markets experienced accelerated sales and elevated market hotness index, whereas areas with higher mortality risk or deeper economic uncertainty saw cooling conditions. In Table 2, the COVID-19 period dummy variable has a mean of 0.43 (std. dev. = 0.49),

indicating that 43% of ZIP code–month observations in the sample occur during the defined pandemic period.

Violent Crime Rate. Elevated violent crime rates are anticipated to dampen housing demand by reducing neighborhood desirability and increasing perceived safety risks, consistent with hedonic pricing models where crime is treated as a negative local amenity in household utility functions (Gibbons, 2004). Empirical evidence shows that buyers require price discounts to offset safety concerns, which slows turnover and reduces market hotness index (Lynch and Rasmussen, 2001; Linden and Rockoff, 2008). Willingness-to-pay estimates suggest that households are prepared to pay a substantial premium to avoid violent crime exposure, reinforcing its role as a salient disamenity in housing market equilibrium (Bishop and Murphy, 2011). I Table 2, the violent crime rate variable has a mean of 401.58 incidents per 100,000 population (std. dev. = 219.32), a minimum of 43.42, a median of 373.19, and a maximum of 1,191.94, indicating substantial variation across ZIP code areas in the sample and a right-skewed distribution driven by high-crime outliers.

Injury Mortality Rate. The county-level injury mortality rate—measured as the number of deaths due to injuries per 100,000 population—includes both intentional causes (e.g., homicide, suicide) and unintentional causes (e.g., motor vehicle accidents, poisonings, drownings, and falls). Elevated rates signal deeper structural vulnerabilities, such as economic distress, occupational hazards, unsafe behaviors, and inadequate emergency medical infrastructure, particularly in rural areas where delayed trauma care is more prevalent (County Health Rankings & Roadmaps – Injury Death). These conditions diminish perceived livability and increase the non-financial cost of residence, making prospective homeowners less willing to purchase properties in affected areas. In ZIP codes located within counties with persistently high injury mortality, such deterrent effects are likely to slow transaction velocity and weaken buyer competition, ultimately reducing the real estate market hotness index. Table 2 shows that the county-level injury mortality rate averages 71.03 deaths per 100,000 (std. dev. = 22.06), with a minimum of 33.11, a median of 69.00, and a maximum of 137.38, indicating a relatively symmetric distribution.

Number of Social Associations. The number of social associations—measured as the number of membership organizations per 10,000 population—serves as a proxy for local social capital and community engagement. Higher levels of social connectivity enhance neighborhood desirability by fostering trust, reciprocity, and collective efficacy (Putnam, 2000; Glanville and Andersson, 2013). These attributes not only increase resident retention but also generate positive network externalities that attract in-migration (Glaeser, Laibson, and Sacerdote, 2002). As a result, stronger community ties can elevate housing demand and accelerate transaction velocity, thereby increasing the real estate market hotness index. In Table 2, the number of social associations has a mean of 9.29 (std. dev. = 3.54), a median of 8.75, a minimum of 3.37, and a maximum of 22.45, indicating substantial variation in community organization density across counties.

Fraction of African American Residents. The fraction of African American residents captures demographic composition, which may correlate with historical patterns of segregation, redlining, and unequal credit access. While demographic composition per se should not affect intrinsic housing demand in a frictionless market, prior evidence suggests that racial composition may be capitalized into prices and market activity through discriminatory buyer preferences and lender behavior (see e.g., Munnell, Browne, and McEneaney, 1996; Ladd, 1998; Bayer, Ferreira, & McMillan, 2007; Devah and Hana, 2008; Rugh and Massey, 2010; Bartlett et al., 2022; Butler, Mayer, and Weston, 2022; Gerardi, Willen, and Zhang, 2023; Chernenko and Scharfstein, 2024; Greenwald et al., 2024; Bhutta, Hizmo, and Ringo, 2025). In Table 2, the fraction of African American residents has a mean of 16.39% (std. dev. = 14.64), a median of 11.20%, and a range from 0.34% to 57.24%, reflecting significant demographic heterogeneity across ZIP codes.

Population Growth. Higher population growth increases housing demand through both owner-occupier and rental channels (Thompson, 1937). This heightened demand reduces inventory duration and raises the real estate market hotness index. The elasticity of hotness with respect to population growth reflects the responsiveness of housing supply. In Table 2, population growth averages 0.43% (std. dev. = 1.17%), with a median of 0.30%, a minimum of –2.20%, and a maximum of 4.10%, indicating substantial variation in local demographic dynamics.

Income Inequality. Income inequality, measured as the ratio of household income at the 80th percentile to that at the 20th percentile, can exert ambiguous effects on real estate market hotness. Elevated inequality may generate a bifurcated housing market, characterized by strong demand for high-end properties but slower turnover in lower-priced segments. Rising incomes among higher earners can exacerbate affordability constraints for lower-income households, potentially dampening activity in the latter segment (Rodda, 1994; Vigdor, 2002). Conversely, if inequality reflects productivity-driven economic dynamism, it may stimulate aggregate housing demand by raising overall purchasing power (Moretti, 2013). The net impact depends on whether inequality primarily displaces middle-income households or coincides with broad-based income growth. In Table 2, income inequality has a mean of 4.76 (std. dev. = 0.68), a median of 4.70, and a range from 3.44 to 7.10, highlighting considerable disparities in income distribution.

Median Household Income. Higher median household income enhances local purchasing power, enabling buyers to bid more aggressively and reduce time-on-market, thereby increasing the real estate market hotness index. In Table 2, median household income is expressed in natural logarithms, with a mean of 10.90 (std. dev. = 0.26), a median of 10.88, a minimum of 10.41, and a maximum of 11.57.

Per Capita Personal Income. Per capita personal income provides a broader measure of local wealth, capturing both labor and non-labor income sources. Higher per capita personal income should similarly increase effective demand for housing and raise market hotness. In Table 2, per capita personal income is expressed in natural logarithms, with a mean of 10.78 (std. dev. = 0.25), a median of 10.75, a minimum of 10.32, and a maximum of 11.59.

Unemployment Rate. A higher unemployment rate undermines household income security and reduces mortgage qualification rates, weakening demand for home purchases. From the supply side, elevated unemployment makes homeowners less willing to move, as their increased risk of job loss and reduced purchasing power limit their ability to buy new homes. It also raises the difficulty of selling existing properties, discouraging listings. Consequently, higher unemployment reduces both the number of buyers and sellers, thinning the housing market and lowering matching quality on average. Combined with an expanded pool of distressed sellers, these factors increase supply without a commensurate rise in demand, thereby putting downward pressure on market hotness (Li, Pengfei, and Qinghua, 2018). In Table 2, the unemployment rate has a mean of 4.96 (std. dev. = 1.90), a median of 4.50, a minimum of 2.20, and a maximum of 11.50.

GDP Growth. Local GDP growth is a macroeconomic indicator of regional economic health. Higher growth rates boost income, employment, and migration inflows. This in turn can stimulate housing demand. As economic activity accelerates, properties tend to sell more quickly, increasing market hotness. This pattern is consistent with the procyclical nature of housing demand documented in the macro-housing literature (Bernanke and Gertler, 1995; Iacoviello and Neri, 2010). In Table 2, GDP growth rates have a mean of 2.06% (std. dev. = 4.05%), a median of 2.40%, a minimum of –9.40%, and a maximum of 13.50%, indicating both severe contractions and robust expansions within the sample period.

Housing Return. Housing returns capture capital gains expectations, which are a key driver of speculative and investment demand. Higher recent returns can generate momentum effects, attracting additional buyers and shortening time-on-market (Case and Shiller, 1989). However, in the presence of supply constraints, rapid appreciation may reduce affordability for marginal buyers, potentially dampening turnover in lower-income segments. In Table 2, monthly housing returns have a mean of 0.04 (std. dev. = 0.06), a median of 0.04, a minimum of –0.10, and a maximum of 0.23, reflecting substantial variation in local housing price dynamics.

Insert Tables 2, 3, and 4

4.2 Baseline Effect on Market Hotness

We examine whether mass shooting incidents significantly alter the perceived attractiveness and competitiveness of the local housing market. Table 5 reports results from our baseline fixed-effects specification, controlling for economic, demographic, and social characteristics. The coefficient on the mass shooting dummy is −1.130 (t = −3.22), indicating that ZIP codes experiencing a mass shooting record a drop in market hotness index of roughly 1.13 points, holding other factors constant. Using the mean market hotness index of 48.71 (SD = 22.09) from Table 2, this represents a 2.32% decline relative to the average—large enough to move a market from a competitive to a more sluggish segment. Given that only 4% of ZIP code–month observations in the sample coincide with a mass shooting, the effect is both statistically and economically meaningful.

Turning to the control variables, the COVID-19 period dummy is negative and significant (−1.253, t = −2.72), consistent with pandemic-era disruptions dampening housing market competitiveness. Higher violent crime rates are associated with lower market hotness index (−0.0126, t = −2.54), reinforcing evidence that safety concerns depress demand. In contrast, the injury death rate enters positively and significantly (0.206, t = 4.22), suggesting that its variation may capture factors—such as rural location or lower population density—that in some contexts are associated with tighter housing markets.

Social associations are negatively related to hotness (−1.614, t = −3.12), contrary to expectations from community capital theories, potentially reflecting that higher association density coincides with smaller or more close-knit communities where turnover is slower. Population growth is strongly positive (2.380, t = 6.11), consistent with demand pressures outpacing supply. Median household income is negative and significant (−23.151, t = −4.05), suggesting that higher-income areas in this sample may already be in more mature markets with slower turnover. Income inequality is positively associated with hotness (2.565, t = 1.81), possibly indicating that economically dynamic areas with high inequality also have strong upper-tier demand.

The unemployment rate is strongly negative (−3.034, t = −11.28), reflecting the adverse impact of job insecurity on market activity. Monthly housing returns show a large positive effect (65.914, t = 14.32), consistent with momentum-driven speculative and investment demand. Other controls, including fraction of African American residents, per capita personal income, and GDP growth rate, are statistically insignificant.

Overall, the evidence indicates that real estate market hotness declines significantly in the aftermath of a mass shooting, even after accounting for a broad set of economic, demographic, and social conditions. In the following sections, we explore whether this decline reflects demand-side channels—such as reduced buyer interest—or supply-side channels, including changes in seller behavior and listing activity.

Insert Table 5

4.3 Demand-Side Mechanisms

To assess whether the observed effects operate through changes in buyer interest, we examine demand-side indicators, including property view activity on Realtor.com and trends in median days on market. A decline in demand-driven measures following a mass shooting would suggest a behavioral response by potential buyers.

Our first measure of the demand mechanism is the demand score of the real estate market—an index ranking a ZIP code's unique listing page viewers per property relative to all other ZIP codes. Higher values of this score indicate stronger buyer interest and greater relative attractiveness of properties in that ZIP code, whereas lower values suggest diminished online engagement and potentially weaker demand conditions (Residential Real Estate Data – Data dictionary). Table 2 shows that the mean (median) demand score is 47.09 (45.58), with substantial cross-sectional

variation: scores range from 1.12 to 98.88, indicating considerable heterogeneity in the relative attractiveness of ZIP-code-level housing markets.

Our second and final measure of the demand mechanism is the listing details page unique viewers per property ratio. This variable is defined as the number of viewers a typical property receives in a given ZIP code within a month divided by the number of viewers a typical property receives nationwide during the same month. Due to the strong positive (right) skewness of this variable—most ZIP codes cluster at relatively low ratios while a small number exhibit very high national attention—we take the natural logarithm of the sum of this ratio and one to compress the right tail and reduce the influence of outliers. Values greater than one indicate that properties in the ZIP code attract above-average national interest, signaling heightened buyer attention and stronger market competitiveness, whereas values below one suggests relatively weaker demand intensity (Residential Real Estate Data – Data dictionary). In Table 2, the mean (median) of this ratio is 0.66 (0.64), with significant variation across ZIP codes: the minimum value is 0.18, and the maximum is 1.37.

We replace the dependent variable of our baseline regression model with the demand-side metrics to examine whether the observed decline in market hotness following a mass shooting operates through reduced buyer interest. The expectation is that mass shooting incidents diminish the willingness of potential buyers to engage with properties in the affected ZIP code, thereby lowering online engagement and weakening market competitiveness.

Column (1) of Table 6 reports results for the demand score, an index ranking ZIP codes by the number of unique listing page viewers per property relative to other ZIP codes. The coefficient on the mass shooting indicator is −1.0945 (t-statistic = −3.05), indicating that mass shooting events are associated with a decline in buyer interest as measured by relative online viewing activity. The estimated coefficient implies that the average ZIP code experiencing a mass shooting sees its demand score fall by 1.09 points. Relative to the mean, this represents a 2.32% decline in the demand score, or roughly a two-percentile drop in the national demand ranking. Although the absolute magnitude may appear modest, the economic significance is amplified by the fact that mass shooting incidents are rare, meaning that even infrequent occurrences can trigger noticeable shifts in market attractiveness when they do occur.

Column (2) presents results for the listing details page unique viewers-per-property ratio, defined as the number of viewers a typical property receives in the ZIP code divided by the number of viewers a typical property receives nationwide in the same month, with the dependent variable specified as the natural logarithm of one plus this ratio. The coefficient on the mass shooting indicator is −0.0101 (t-statistic = −3.33), indicating that a mass shooting is associated with a 1.01% decline in the viewers-per-property ratio. Given the mean value of 0.66 in Table 2, this corresponds to an average absolute decline of about 0.0067 viewers per property relative to the national benchmark. This effect, while modest in magnitude, remains meaningful given the relatively short horizon over which buyer attention is typically measured in online search activity.

Overall, the results in Table 6 indicate that mass shooting events measurably reduce online engagement with listings in the affected ZIP codes. The decline in both the relative demand score and the viewers-per-property ratio is consistent with potential buyers avoiding markets perceived as riskier or less desirable in the aftermath of such events. These patterns point to a plausible role for demand-side forces in driving the reduction in baseline hotness documented earlier. A more detailed examination of possible supply-side adjustments is presented in the next section.

Insert Table 6

4.4 Supply-Side Mechanisms

We examine real estate supply-side responses to mass shooting incidents by analyzing changes in transaction speed, the propensity to list new properties, and the likelihood of selling at a discount. Slower transaction speeds may reflect either reduced buyer competition or greater seller caution. Declines in new listings can indicate shifts in seller expectations, with potential sellers postponing market entry until uncertainty diminishes. An increased propensity to sell at a discount may signal motivated or distressed selling in response to perceived demand weakness or anticipated price depreciation. While discounting is often triggered by lower demand, the decision to accept a discounted offer is inherently a seller-side choice, making it a relevant supply-side channel. By jointly assessing these dimensions, we evaluate whether mass shootings prompt temporary market withdrawal, accelerate sales to preempt further price declines, or leave seller behavior largely unchanged.

Our first supply-side mechanism measure is the supply score, an index ranking the median number of days property listings spend on the market within the ZIP code during a month relative to other ZIP codes in the same month. The lower the days on market, the higher the supply score (Residential Real Estate Data – Data Dictionary). A high supply score reflects stronger market competitiveness, as properties transact quickly, indicating that sellers face favorable conditions to convert listings into sales without significant price concessions. Conversely, a decline in the supply score suggests weaker market competitiveness, as properties take longer to sell, potentially forcing sellers to adjust expectations, extend listing durations, or become less willing to reduce asking prices to attract buyers. In Table 2, we find that the mean (median) supply score is 50.33 (49.60), with substantial variation ranging from a low of 1.17 to a high of 98.66.

Our second supply-side mechanism is the median days on market metric, defined as the median number of days that property listings remain active within a ZIP code in a given month, scaled by the corresponding U.S. median for the same month. This scaling controls for national-level seasonality and macroeconomic conditions (Residential Real Estate Data – Data Dictionary). Because the distribution of this metric is right-skewed, we apply a natural logarithmic transformation to one plus the scaled value. As reported in Table 2, the mean (median) of this variable is 3.96 (3.97), indicating that, on average, properties in a given ZIP code take roughly four times longer to sell than the national median.

Our third supply-side measure is New Listings, defined as the ratio of properties newly added to the market within a ZIP code–month to the total number of active and pending listings in that same ZIP code–month (Residential Real Estate Data – Data Dictionary). This metric captures the inflow of new inventory relative to the existing stock, with higher values indicating more intense listing activity. Observations with ratios exceeding one, reflecting data errors, are excluded from the analysis. As shown in Table 2, the mean (median) share of newly listed properties is 0.33 (0.30), with substantial variation across ZIP codes and over time. The distribution ranges from 0.00 to 0.86, underscoring pronounced heterogeneity in the intensity of new listing activity.

Our fourth and final supply-side measure is the Share of Listings with Reduced Sale Price, defined as the ratio of the number of listings that have undergone a price reduction within a ZIP code in a given month to the total number of listings in the same ZIP code and month (Residential Real Estate Data – Data Dictionary). Higher values of this ratio indicate a greater prevalence of price adjustments, potentially reflecting weaker demand conditions or increased seller urgency. As reported in Table 2, the mean (median) share of listings with reduced sale price is 0.17 (0.16). The measure exhibits considerable heterogeneity across ZIP codes and time, ranging from a minimum of 0.00 to a maximum of 0.51.

To examine the effect of mass shooting incidents on the supply side of the real estate market, we re-estimate our baseline specification using four alternative supply-side measures as dependent variables: the supply score, median days on market, new listings, and the share of listings with reduced sale prices. The results, reported in Table 7, are consistent with the expectation that such incidents tighten supply conditions. The coefficient on the mass shooting indicator in the supply score regression is –1.1447 (t = –2.29), implying that, relative to the mean supply score of 50.33 (SD = 27.13) from Table 2, supply declines by approximately 2.27%. This magnitude corresponds to a downward shift of roughly one percentile in the national distribution of supply scores, indicating a meaningful post-event contraction in market competitiveness.

In addition, the effect on median days on market is positive and statistically significant: the coefficient is 0.0158 (t = 2.30). Given the mean value of 3.96, this translates into a 0.40% increase in time on market. While the magnitude is modest, the result indicates that properties in affected ZIP codes take slightly longer to sell following a mass shooting, consistent with reduced buyer urgency or heightened seller caution.

Turning to new listings, the estimated coefficient is –0.0069 (t = –2.34). Given a mean share of newly listed properties of 0.33 (SD = 0.16), this represents a 2.09% decline in new inventory. This finding is consistent with a measurable withdrawal of potential sellers from the market in the aftermath of a mass shooting, potentially reflecting uncertainty about demand conditions or reluctance to transact in a perceived high-risk environment.

For the share of listings with reduced sale prices, the coefficient is 0.0043 (t = 2.03), implying a 2.53% increase relative to its mean of 0.17 (SD = 0.10). This suggests that sellers who remain

active in the market post-event are more willing to offer price reductions, potentially to stimulate demand or to offset negative buyer perceptions in the wake of the incident.

Overall, these results point to a supply-side response characterized by statistically significant reductions in market competitiveness, slower sales, fewer new listings, and a higher prevalence of price concessions. This pattern is consistent with a post-event environment in which sellers are both more reluctant to enter the market and more inclined to adjust prices downward to achieve a sale.

Insert Table 7

4.5 Price Effects

We next examine whether mass shooting incidents influence local housing market valuations, as reflected in listing prices. The evidence from Section 3.3 shows that such events reduce buyer interest, with declines in both the demand score and the viewers-per-property ratio, indicating diminished online engagement and market competitiveness. On the supply side (Section 3.4), we find a contraction in new listings and an increase in the share of listings with reduced sale prices, consistent with fewer sellers entering the market and greater willingness among those who remain to offer concessions. These adjustments exert opposing pressures on prices, with weaker demand pushing valuations downward and tighter supply offsetting part of this effect.

We measure the price effect using the median listing price per square foot within a ZIP code during a given month, deflated by the GDP implicit price deflator (base year 2017) to remove the influence of inflation. As reported in Table 2, the mean (median) deflated median listing price is $183.92 ($136.67) per square foot, with substantial cross-sectional and time variation ranging from $43.00 to $952.12.

To examine the effect of mass shooting incidents on property prices, we use the baseline regression model but replace the real estate Market hotness index with the median listing price per square foot. Table 8 reports the results of this analysis. The coefficient on the mass shooting indicator is −2.0687 and statistically significant at the 5% level ($t = -2.14$). This estimate implies that, following a mass shooting, the median listing price per square foot in the affected ZIP code declines by approximately $2.07, holding other factors constant. Relative to the sample mean deflated listing median price of $183.92 per square foot (Table 2), this effect represents about a 1.13% decrease in valuations. Although modest in percentage terms, such a shift is economically meaningful in local housing markets, where prices tend to adjust slowly and small per–square foot changes can translate into substantial property-level value losses. The high adjusted R^2 of 0.98 reflects the strong explanatory power of the fixed effects in capturing location-specific attributes and common temporal shocks, indicating that the estimated effect is identified within a narrow residual band.

Overall, the results show that weaker buyer interest, as documented in Section 3.3, outweighs any upward price pressure from reduced supply (Section 3.4), leading to a measurable post-event decline in listing prices.

Insert Table 8

4.6. Robustness and Additional Checks

This section reports additional analyses that probe identification, quantify dynamics, and test mechanism-consistent heterogeneity. We begin with design and placebo validations, then examine the persistence and informational content of mass-shooting events. We next assess heterogeneity across information environments, belief priors, and behavioral conditions, and conclude by addressing the standard concern that our estimates reflect endogenous mortgage-credit contractions rather than housing-demand responses.

4.6.1. Identification and Design Robustness

We first examine whether our baseline estimates are sensitive to key design choices. In the baseline analysis, treated ZIP codes are matched to control ZIP codes using nearest-neighbor propensity score matching with three neighbors. To assess sensitivity, we re-estimate the baseline specification while varying the number of nearest neighbors from one to ten. As reported in Table 9, the estimated mass-shooting effect remains negative and statistically significant across all matching ratios, with coefficients tightly clustered around the baseline estimate. This stability indicates that the main result is not driven by a particular matching configuration.

Insert Table 9

Second, we assess whether the estimated effect could plausibly arise from chance correlations between treatment assignment and housing-market outcomes. We conduct an in-space placebo analysis by randomly reassigning treatment to ZIP codes that did not experience a mass shooting and re-estimating the model 1,000 times. The placebo distribution of coefficients, shown in Figure 3, is centered near zero, while the true estimate lies in the left tail. The implied left-sided placebo p-value is 0.033, indicating that fewer than 3.3% of placebo assignments generate an estimate as negative as the observed effect. This evidence suggests that the decline in market hotness is unlikely to reflect random variation.

Insert Figure 3

Third, we verify that the results are not an artifact of pandemic-era shocks. Although the baseline specification includes a COVID-19 indicator, pandemic dynamics may not be fully absorbed by a single dummy. We therefore exclude all observations from 2020–2021 and re-estimate the baseline model. As reported in Table 10, the mass-shooting coefficient remains negative and statistically

significant, with an economic magnitude comparable to the baseline. This evidence indicates that our findings are not driven by COVID-era confounding.

Insert Table 10

4.6.2. Additional Checks: Heterogeneity Across Local Conditions

To assess whether the effect of a mass shooting on housing market hotness varies systematically with local market conditions, we investigate heterogeneity across subgroups defined by socio-economic and demographic characteristics. Prior studies have identified structural factors that elevate the likelihood of such events. For example, areas with high income inequality can foster environments of frustration, resentment, and perceived injustice, which may contribute to the emergence of mass shootings (Cabrera and Kwon, 2018; Kwon and Cabrera, 2019). Similarly, the probability of such incidents rises when individuals are under acute financial or economic distress, a condition more prevalent in communities with lower economic growth, higher unemployment, and lower household income—often used as a proxy for poverty (Brodeur and Yousaf, 2025). Gun violence, and by extension the potential for mass shootings, is also elevated in areas with greater access to firearms, particularly where illegal gun markets thrive or firearm regulations are more lenient (Stolzenberg and D'Alessio, 2000; Morrison et al., 2021).

Demographic and social cohesion factors further influence mass shooting risk. Research indicates that African American minority communities with higher levels of legal cynicism—where residents perceive the law as illegitimate or ineffective—experience higher rates of gun violence, which may extend to mass shootings (Kirk and Papachristos, 2011). Mass shootings are also more common in areas with lower levels of social integration, where weak community bonds may reduce informal social control (Kwon and Cabrera, 2018). Additionally, the overall prevalence of violence and weapon availability in a community, captured by indicators such as gun violence rates and injury death rates, serves as a proxy for the underlying risk environment. These patterns suggest that local socio-economic disadvantage, demographic composition, social fragmentation, and broader violence exposure shape the likelihood of mass shooting events, providing a framework for examining how their housing market effects might differ across communities.

We implement a three-step heterogeneity test to assess whether the housing-market response to a mass-shooting incident varies with pre-existing local conditions. Our set of indicators covers labor-market strength (unemployment rate), macro momentum (GDP growth), income distribution (income inequality), purchasing power (median household income), baseline safety (violent-crime rate and injury-related death rate), the firearm environment (access to firearms and regulatory stringency), demographic composition (share of Non-Hispanic African Americans), and community capital (social associations). In the first step, we sort observations from the baseline sample into two subsamples. The first subsample consists of ZIP code–month observations where the risk of a mass-shooting incident is higher: (1) unemployment above the sample median, (2) GDP growth below the sample median, (3) income inequality above the sample median, (4) median household income below the sample median, (5) violent-crime rate above the sample median, (6) injury-related death rate above the sample median, (7) access to firearms above the sample median, (8) firearm-regulatory leniency above the sample median, (9) share of Non-Hispanic African Americans below the sample median, and (10) social associations below the sample median. The second subsample consists of the opposite (lower-risk) cases. We form these two subsamples for

each indicator based on its sample median. In the second step, we re-estimate the baseline regression model separately within each subsample. In the final step, we test whether the mass-shooting coefficient differs across the two subsamples; the null hypothesis is that the mass-shooting coefficients are equal across subsamples. Table 11 reports the results.

Table 11 shows that our finding of a uniform negative response of housing-market hotness to mass-shooting incidents is robust to heterogeneity in pre-existing local conditions. Across all moderators and in both panels, the mass-shooting coefficient remains negative and of comparable magnitude. Cross-panel equality tests are rarely rejected at conventional levels, which indicates broadly similar effects in high- and low-risk environments. The only suggestive deviation arises for firearm-regulation stringency, where areas with stricter laws exhibit a somewhat larger decline; this difference is marginal and should be interpreted cautiously. Overall, the evidence indicates that the observed decline in market hotness is not an artifact of underlying socio-economic, demographic, or institutional conditions, but rather reflects a generalized demand response to mass-shooting events.

Insert Table 11

4.6.3. Dynamic Effects and Persistence

Understanding whether the economic consequences of mass shootings are transitory or persistent is central to assessing their welfare and market implications. While an immediate decline in housing demand following a highly salient violent event is intuitive, the duration of this response is theoretically ambiguous. On the one hand, heightened fear, media attention, and perceived neighborhood risk may dissipate quickly as information decays. On the other hand, mass shootings may induce longer-lasting revisions in household beliefs, neighborhood stigma, or expectations about local safety, leading to sustained changes in housing-market activity.

Accordingly, identifying persistence requires moving beyond contemporaneous or short-run estimates and tracing the cumulative response of housing-market conditions over time. Our objective is therefore not only to test whether the effect of mass shootings is statistically detectable in the months following an event, but also to quantify how economically meaningful these effects remain as the horizon expands.

To study persistence, we estimate a sequence of distributed-lag specifications that trace the dynamic response of housing-market hotness in the months following a mass shooting. The outcome variable is Realtor.com's Market Hotness Index at the ZIP–month level. The treatment variable, masshooting, is a binary indicator equal to one if a mass shooting occurred in the ZIP code in the previous month; thus, the contemporaneous coefficient captures the effect one month after the event.

We estimate models of the form (2):

$$\text{Market hotness index}_{z,t} = \sum_{j=0}^{H-1} \beta_j \, Mass\ shooting_{z,t-j} + X'_{z,t}\,\gamma + \alpha_z + \delta_t + \varepsilon_{z,t} \quad (1),$$

where H ∈ {2,4,6,8,10,12}) denotes the horizon in months after the event, $X_{z,t}$ is a rich set of one-year lagged local economic and demographic controls, except for the one-month lagged housing return. The α_z and δ_t are ZIP and month fixed effects. Standard errors are clustered at the ZIP-code level.

For each horizon, we focus on the cumulative effect $\sum_{j=0}^{H-1} \beta_j$, which measures the total impact of a mass shooting on housing-market hotness over the first H months following the event. This approach avoids overinterpreting individual lag coefficients and aligns naturally with the concept of economic persistence. To assess economic significance, we benchmark cumulative effects against the mean and standard deviation of the hotness index computed over the regression sample. We assess economic significance based on the magnitude of the estimated cumulative effects relative to the distribution of the outcome, independent of statistical significance. We report the results of this analysis in Table 12.

The results reveal a clear pattern of economically meaningful persistence. At short horizons, the cumulative decline in housing-market hotness is already substantial. Within two months of a mass shooting, the hotness index falls by approximately 1.7 points, corresponding to about 3.5% of the sample mean (roughly 0.08 standard deviations). By four months, the cumulative effect deepens to 3.2 points, or 6.6% of the mean (0.15 standard deviations).

The magnitude of the response continues to grow over medium horizons. At six months after the event, the cumulative decline reaches 4.7 points, equivalent to nearly 10% of the mean and 0.21 standard deviations. Extending the horizon further yields diminishing incremental effects but little evidence of reversal. At eight and ten months, the cumulative decline stabilizes around 0.26–0.28 standard deviations, and by twelve months the total effect approaches 0.32 standard deviations, or roughly 15% of the mean hotness level.

Two features of these results are noteworthy. First, persistence manifests primarily through accumulation rather than prolonged significance of individual monthly coefficients. Later-month effects are noisier, yet the cumulative impact remains large, underscoring the importance of evaluating dynamics jointly. Second, while the economic effect grows through the first six to eight months, it begins to plateau thereafter, suggesting that mass shootings induce a medium-run cooling of local housing markets rather than a permanently escalating decline.

Overall, the persistence analysis indicates that the housing-market consequences of mass shootings are neither fleeting nor purely short-lived. Instead, mass shootings generate a sustained reduction in market hotness that builds over several months and remains economically large for at least half a year. By six months after an event, local housing markets are substantially cooler, and this deterioration persists through the one-year horizon with limited signs of recovery.

These findings complement our event-time and placebo analyses by demonstrating that the observed decline in housing-market activity is not a transitory shock or a statistical artifact. Rather, mass shootings have durable economic effects that meaningfully alter local housing-market conditions well beyond the immediate aftermath of the event.

Insert Table 12

4.6.4. Treatment Definition Robustness: A Facts-Based Mass Shooting Severity Index

A natural concern in estimating the effect of mass shootings on local housing-market conditions is that a binary indicator for the occurrence of an incident may obscure economically meaningful heterogeneity in the severity, intensity, and informational content of violent events. If housing-market participants respond to perceived risk and belief-relevant information rather than mechanically to the presence of violence, then the housing-market response should vary systematically with observable features of the incident—such as the extent of human harm, whether the threat is fully resolved, and whether exposure is repeated within a short time window.

To address this concern in a unified and transparent manner, we replace the binary mass-shooting indicator with a facts-based Mass Shooting Severity (MSS) index. The MSS index consolidates multiple dimensions of incident severity into a single measure using only verifiable, incident-level facts recorded by the data provider. Importantly, the index does not rely on media coverage, news tone, or narrative framing, which may be endogenous to local conditions or correlated with housing-market activity. Instead, the MSS index is designed to capture the objective informational content of a mass shooting that is most likely to influence household beliefs about neighborhood safety and long-run residential desirability.

The MSS index is constructed at the incident level in three steps. First, we measure realized human harm, which constitutes the most direct and salient signal of local risk. Harm is defined as a weighted sum of casualties, with fatalities receiving greater weight than injuries to reflect the discrete escalation in perceived severity associated with loss of life. To prevent extreme events from mechanically dominating the index, we apply a logarithmic transformation:

$$H_i = \log(1 + 2\,VictimsKilled_i + VictimsInjured_i).$$

This transformation preserves the ordinal ranking of incidents while imposing diminishing marginal effects of additional casualties, consistent with standard practice in the disaster and conflict literatures.

Second, we capture uncertainty about ongoing risk using information on suspect outcomes. Specifically, we define an indicator equal to one if no suspect is arrested, killed, or injured during the incident. Such unresolved events plausibly generate heightened ambiguity and sustained fear, as residents cannot rule out continued threat. This component captures the informational content associated with uncertainty rather than realized harm.

Third, we account for incident closure, measured as

$$C_i = \log(1 + SuspectsArrested_i + 0.5(2\ VictimsKilled_i + VictimsInjured_i)),$$

which reflects the extent to which the immediate threat is neutralized through law enforcement action. Closure attenuates perceived ongoing risk but does not offset harm already realized.

We combine these components into a latent incident-level severity measure,

$$MSS_i^{raw} = H_i + \lambda_i\ U_i\ - \lambda_c\ C_i,$$

Where $\lambda_U = 0.3$ and $\lambda_C = 0.2$ are conservative weights chosen to ensure that uncertainty and closure adjust perceived severity without dominating realized harm. Because these components are allowed to offset one another, MSS^{raw} can in principle take negative values when harm is limited and closure is immediate.

Negative values, however, are not economically meaningful in the context of housing-market responses. A negative severity value would imply that a mass shooting constitutes a "better-than-baseline" shock relative to a ZIP–month with no shooting, which is not a plausible counterfactual. Accordingly, we define the final Mass Shooting Severity index as

$$MSS_i = \max\{0, MSS_i^{raw}\},$$

so that zero uniquely corresponds to the absence of a mass shooting, while positive values capture increasing severity and informational content.

We aggregate the incident-level MSS to the ZIP–month level by summing across all mass shooting incidents occurring within a ZIP code in a given month:

$$MSS_{z,i} = \sum_{i\in(z,)} MSS_i$$

This aggregation captures cumulative severity and treatment intensity in a transparent manner and preserves the property that ZIP–months without any mass shootings receive $MSS_{z,i} = 0$ by construction. Summation is economically natural in this setting, as multiple incidents within a short horizon represent a larger informational and behavioral shock than a single event. Appendix 2 provides a detailed description of the construction of the Mass Shooting Severity (MSS) index, including its component measures, functional form, and aggregation to the ZIP–month level.

Table 13 reports estimates from specifications in which the MSS index replaces the binary mass-shooting indicator. The dependent variable is the Market Hotness Index measured at the ZIP–month level. All regressions include ZIP fixed effects, year–month fixed effects, and the full set of predetermined economic, demographic, and social controls. The estimated coefficient on the MSS index is negative and highly statistically significant, indicating that housing-market hotness

declines monotonically with the objective severity and cumulative intensity of mass shooting exposure.

These results demonstrate that housing markets respond smoothly to the severity of violent shocks rather than exhibiting a threshold response to their occurrence. By consolidating severity gradients, treatment intensity, and incident heterogeneity into a single facts-based measure, the MSS index subsumes earlier robustness tests based on casualty counts, repeated exposure, and contextual distinctions. The findings therefore confirm that our baseline results are not driven by a coarse treatment definition or by a small number of extreme events, but instead reflect belief-relevant variation in the informational content of mass shootings.

Insert Table 13

4.6.5. Information Salience: Public Attention

Our empirical analysis relies on mass-shooting data from publicly available sources, implying that potential homebuyers may become informed about these events through media coverage. Whether and when such information reaches homebuyers is therefore relevant for understanding housing-market responses following a mass shooting.

We proxy for the informativeness of potential homebuyers using an indicator for high public attention to mass shootings. This variable equals one if a state's monthly public interest in mass shootings exceeds the contemporaneous national average, and zero otherwise. By construction, the indicator identifies months in which a state receives above-average public attention relative to other states, net of aggregate time variation in attention. This normalization removes common national fluctuations in news intensity and search behavior, allowing us to isolate cross-state differences in salience that are relevant for identification. Public attention is measured using data from Google Trends. We retrieve information on mass-shooting–related web searches using the Google Trends *topic* "Mass shootings," rather than specific search terms. Using topics aggregates all relevant search queries across languages within a state and is therefore more representative of overall public interest than language-specific search terms (Google Trends Help, 2025).

We extend the baseline specification by including one-month lagged high-public-attention indicator and its interaction with the mass-shooting indicator. This specification allows the association between mass shootings and housing-market conditions to vary with the level of contemporaneous public attention.

Table 14 reports the results. The interaction between Mass Shooting and High Public Attention to Mass Shootings is negative and statistically significant, indicating that the association between mass shooting incidents and housing-market conditions is more adverse in periods of elevated public attention. The coefficient on the mass-shooting indicator captures the association in lower-attention months, while the interaction term measures the incremental difference when public attention is high. The negative interaction therefore implies that the combined effect of a mass shooting is more negative when public attention exceeds its contemporaneous national average.

To summarize the overall effect, we report the average marginal effect of a mass shooting evaluated at the sample means. This estimate is negative, economically meaningful, and statistically significant. The joint *F*-test strongly rejects the null that the coefficients on the mass-shooting indicator, the high-public-attention indicator, and their interaction are jointly equal to zero. Taken together, these results indicate that the housing-market response to mass shootings varies systematically with public attention, with larger deteriorations observed when incidents receive elevated relative attention.

Insert Table 14

4.6.6. Belief Heterogeneity: Political Composition

The housing-market consequences of mass shootings may depend not only on the occurrence of an incident itself, but also on how local residents perceive and process information about gun violence. Political party affiliation and ideology are systematically associated with attitudes toward firearms, tolerance for gun-related risk, and beliefs about public safety and regulation (Spitzer, 2020). Importantly, mass shootings are highly politicized events in the United States, and the information households receive about such incidents is shaped by local media and political environments. Gentzkow and Shapiro (2010) show that newspaper content responds to audience ideology and that media slant varies systematically with local political composition: using ZIP-code-level circulation data, they document that right-leaning newspapers circulate relatively more in ZIP codes with a higher share of Republican voters, while left-leaning newspapers show the opposite pattern, even within narrowly defined geographic markets. This evidence implies that residents in politically different areas are exposed to systematically different framings of the same violent events. As a result, households may update beliefs about neighborhood safety and long-run residential desirability in systematically different ways following a mass shooting, generating heterogeneous housing-market responses across political contexts.

Motivated by this insight, we examine whether the housing-market response to mass shootings varies systematically with local political composition. We do not interpret political vote shares as measures of political behavior per se, but rather as proxies for heterogeneity in risk perceptions, media consumption, and belief updating across communities. If mass shootings affect housing markets primarily through an information-based channel—by altering perceptions of safety and long-run neighborhood desirability—then their impact should be stronger in areas where such information is more salient and more readily incorporated into household decision-making. Conversely, in environments where gun violence is perceived as less informative or less salient, the housing-market response should be attenuated.

To test this hypothesis, we augment our baseline specification by interacting the mass-shooting indicator with one-month-lagged local political composition. The dependent variable remains the Market Hotness Index measured at the ZIP–month level. All specifications include ZIP fixed effects and year–month fixed effects, as well as the full set of predetermined economic,

demographic, and social controls described in Appendix A. Standard errors are heteroskedasticity-robust and clustered at the ZIP-code level.

Table 15 reports the results. The estimated coefficient on the mass-shooting indicator is negative and economically meaningful, indicating that mass shootings lead to a pronounced cooling of local housing-market conditions. At the same time, the interaction term reveals systematic heterogeneity in this response across political environments. Specifically, the adverse housing-market effect of a mass shooting is attenuated in areas with higher Republican vote shares, implying that the magnitude of the response varies with local political composition.

Evaluated at the sample means of the covariates, the average marginal effect of a mass shooting remains negative and statistically significant, confirming that mass shootings reduce housing-market hotness on average across locations. However, the marginal effect declines monotonically with local Republican vote share, becoming substantially smaller in more Republican-leaning areas. The final row of Table 15 reports an F-test of joint significance for the mass-shooting indicator, political composition, and their interaction; the strong rejection of the null hypothesis confirms that political composition is an important moderator of the housing-market response to mass shootings.

Taken together, these results indicate that mass shootings do not generate uniform housing-market effects across locations. Instead, their economic impact depends on local information environments and belief updating, consistent with an information-based mechanism rather than a purely mechanical or short-lived demand shock.

Insert Table 15

4.6.7. Children's Long-Run Prospects: Child Opportunity Benchmarks

A central feature of mass shootings in the United States is that a substantial share occur in or around schools and involve child victims. As a result, such events may convey particularly salient information about child safety and long-run developmental environments, beyond their implications for general neighborhood crime. For households with children—or those planning to have children—mass shootings may therefore directly affect beliefs about whether a location can continue to support children's well-being and future prospects. Motivated by this channel, we examine whether the housing-market response to mass shootings varies systematically with local child opportunity, reflecting the extent to which information about children's potential is priced into residential decision-making.

Child opportunity is measured using a county–year Child Opportunity Index ratio defined as the ratio of the state-normed Child Opportunity Index to the nationally normed Child Opportunity Index. This ratio does not capture absolute levels of child opportunity. Instead, it reflects how a county's opportunity standing differs across two reference frames: relative to other counties within the same state and relative to the national distribution. A higher value of the ratio indicates that a county appears more favorable when evaluated against its within-state comparison set than when

evaluated against the national benchmark, whereas a lower value indicates the opposite. This distinction is economically meaningful because many determinants of child opportunity—such as school quality, education finance, and public investment—are shaped at the state level, making within-state comparisons particularly salient for households considering residential choices, especially for local or in-state movers. At the same time, nationally mobile households may rely more heavily on nationwide benchmarks. The ratio therefore captures which comparison frame is relatively more favorable for a given county and year.

The Child Opportunity Index ratio is entered into the baseline regression model lagged by one year. This lag structure ensures that the measure reflects pre-existing opportunity conditions rather than contemporaneous housing-market responses or post-shooting residential sorting, mitigating concerns about simultaneity and reverse causality. Moreover, households plausibly rely on lagged and persistent indicators of neighborhood quality when forming expectations about long-run opportunity, making the lag consistent with the information set used in residential decision-making following a violent shock.

To examine heterogeneity, we augment the baseline specification by including the lagged Child Opportunity Index ratio and its interaction with an indicator for mass shooting incidents. The dependent variable is the Market Hotness Index measured at the ZIP–month level, and all regressions include ZIP fixed effects, year–month fixed effects, and a comprehensive set of predetermined economic, demographic, and social controls. The interaction allows the marginal effect of a mass shooting to vary with the relative importance of within-state versus national opportunity standing.

The sign of this moderating effect is a priori ambiguous. A positive interaction coefficient implies attenuation: mass shootings have a smaller negative impact on housing-market conditions in counties where child opportunity appears relatively stronger under the within-state benchmark. This pattern is consistent with a resilience mechanism in which favorable long-run local attributes—such as strong schools and child-centered public investments—anchor household expectations and dampen the response to negative safety information related to children. In contrast, a negative interaction coefficient would imply amplification, indicating that mass shootings have larger effects in areas where the perceived stakes of child opportunity are high. Allowing for both possibilities provides a flexible test of how households weigh child safety shocks against long-run opportunity considerations.

The results reported in Table 16 show that mass shootings lead to a statistically and economically significant deterioration in local housing-market conditions. The lagged Child Opportunity Index ratio enters positively, indicating stronger baseline housing demand in counties whose opportunity standing is relatively more favorable under the within-state comparison. Importantly, the interaction between the lagged Child Opportunity Index ratio and the mass-shooting indicator is positive and statistically significant. This finding is consistent with the expected attenuation channel: in counties where child opportunity is perceived as relatively strong within the state,

households are less likely to interpret a mass shooting—even one involving a school—as evidence of persistent deterioration in the local environment for children. As a result, the negative housing-market response to mass shootings is smaller in these areas. Nevertheless, the average marginal effect of a mass shooting remains negative and statistically significant, implying that even high-opportunity counties experience meaningful housing-market weakening following such incidents. A joint F-test rejects the null hypothesis that the coefficients on the mass-shooting indicator and its interaction are jointly equal to zero, confirming that child opportunity benchmarks play a first-order role in shaping housing-market responses.

Overall, this result implies that mass shootings convey information about child safety and developmental environments that is capitalized into housing markets. By exploiting county–year variation in how child opportunity is evaluated across geographic reference frames, the analysis shows that housing-market responses depend on whether households view violent incidents as transitory shocks or as signals of persistent risk to children's future prospects.

Insert Table 16

4.6.8. Institutional Priors: State Gun Law Weakness

The housing-market consequences of mass shootings may depend not only on the occurrence of an incident itself, but also on the institutional environment in which the incident takes place. State gun laws shape the baseline risk environment and influence households' expectations about the likelihood and preventability of extreme gun violence. In jurisdictions with weaker gun regulations, residents may perceive mass shootings as more consistent with prevailing background risk, reflecting an environment in which exposure to gun violence is more anticipated. By contrast, in states with stronger gun laws, households may hold stronger priors that such incidents are rare, given existing institutional safeguards.

From a belief-updating perspective, a mass shooting occurring in a strong-law state may therefore constitute a larger informational shock. Standard belief-based models predict that agents respond more strongly to adverse events that violate established priors or occur in environments perceived as relatively safe, because such events induce disproportionate revisions in perceived risk rather than incremental updates (Barberis, Shleifer, and Vishny, 1998; Gennaioli and Shleifer, 2018). In this setting, a mass shooting may prompt a sharper reassessment of neighborhood safety and long-run residential desirability, leading to a larger deterioration in housing-market conditions. By contrast, in weak-law environments where baseline expectations of gun-related risk are higher, the same event carries less incremental informational content and therefore generates a more muted housing-market response. This analysis treats gun laws as institutional priors that shape how households interpret violent shocks, rather than as indicators of political preferences or policy debate.

If mass shootings constitute a larger informational and psychological shock in states with stronger gun laws, the adverse effect of a shooting on housing-market hotness should be correspondingly

larger in those environments and attenuated where gun laws are weaker. In settings with weaker gun regulations, baseline expectations of gun-related risk are higher, reducing the incremental informational content of a mass shooting. Accordingly, we hypothesize a positive interaction between state gun law weakness and mass shooting incidence, implying that the negative housing-market impact of a mass shooting diminishes as gun laws become weaker.

To test this hypothesis, we augment our baseline ZIP–month fixed-effects specification by including a one-year lagged continuous measure of state gun law weakness and its interaction with an indicator for mass shooting incidents. The dependent variable is the Market Hotness Index measured at the ZIP–month level. All regressions include ZIP fixed effects, year–month fixed effects, and the full set of predetermined economic, demographic, and social controls described in Appendix A. Standard errors are heteroskedasticity-robust and clustered at the ZIP level.

The coefficient on the mass shooting indicator captures the effect of a shooting in a state with the strongest gun laws, while the interaction term measures how this effect varies as gun laws become weaker. We additionally report the average marginal effect of a mass shooting, evaluated at the sample means of the covariates, and conduct a joint significance test of the mass shooting indicator and its interaction term.

Table 17 shows that the coefficient on the mass shooting indicator is negative and highly statistically significant, confirming that mass shootings lead to a substantial deterioration in local housing-market conditions. This finding is consistent with our baseline results and indicates a pronounced cooling of housing-market hotness following these events.

The main effect of state gun law weakness is positive but statistically insignificant, suggesting that differences in gun law regimes do not systematically affect housing-market conditions in the absence of a mass shooting. In contrast, the interaction between state gun law weakness and mass shootings is positive and statistically significant at the 5% level. This result implies that the negative effect of mass shootings on housing-market hotness is attenuated in states with weaker gun laws and amplified in states with stronger gun regulations.

Importantly, this pattern is consistent with both rational belief updating and behavioral responses. In states with strong gun laws, mass shootings may generate a sharper reassessment of neighborhood safety and institutional effectiveness, leading to heightened buyer caution and a stronger pullback in housing demand. In states with weaker gun laws, where baseline expectations of gun violence may already be elevated, mass shootings may carry less incremental informational content, resulting in a more muted market response.

The average marginal effect of a mass shooting remains negative and statistically significant, indicating that mass shootings reduce market hotness on average across institutional environments. The joint significance test further rejects the null that the mass shooting indicator and its interaction term are jointly equal to zero, underscoring the importance of state-level gun law regimes in shaping housing-market responses.

Overall, these findings highlight the role of belief formation and behavioral responses in mediating the economic effects of mass shootings. The housing-market impact of mass shootings is not uniform but depends critically on whether the event violates residents' prior expectations about safety and regulatory protection. Mass shootings occurring in states with stronger gun laws appear to generate larger behavioral and informational shock, leading to a more pronounced cooling of local housing markets. This evidence reinforces the view that violent shocks affect economic outcomes not only through objective risk, but also through expectation-based and psychological channels.

Insert Table 17

4.6.9. Mobility and Place Attachment: Net Migration

Households' housing-market responses to mass shootings may depend on local population mobility, which shapes attachment to place, planning horizons, and the interpretation of neighborhood-level shocks. Prior work emphasizes that residential stability fosters place attachment and longer-term investments in local amenities, whereas high population turnover weakens attachment and reduces sensitivity to localized shocks (DiPasquale and Glaeser, 1999; Glaeser and Gyourko, 2005). In areas characterized by high population churn, residents may therefore be less anchored to neighborhood-specific conditions, potentially dampening behavioral responses to adverse events. In contrast, in more stable communities, violent shocks may carry stronger informational content about long-run neighborhood safety and desirability, prompting more pronounced reassessments of housing demand.

These considerations imply that migration dynamics may moderate how housing markets respond to mass shootings. If short-term population turnover weakens local attachment and shortens planning horizons, the negative effect of mass shootings on housing-market hotness should be attenuated in ZIP codes with higher temporary net migration (Haurin and Gill, 2002; Bayer, Ferreira, and McMillan, 2007). Alternatively, permanent net migration reflects longer-run residential sorting in response to local amenity bundles, consistent with classic models of "voting with one's feet" (Tiebout, 1956). We test for these differential moderation effects using separate measures of temporary and permanent net migration.

We extend the baseline ZIP–month fixed-effects specification by including one-month lagged temporary net migration and one-month lagged permanent net migration, along with their interactions with the mass-shooting indicator. The dependent variable is the Market Hotness Index. All specifications include ZIP fixed effects, year–month fixed effects, and the full set of controls described in Appendix A. Standard errors are clustered at the ZIP level. The interaction terms identify whether the housing-market response to a mass shooting varies with prior migration conditions.

Table 18 reports the estimates. The coefficient on the mass-shooting indicator is negative and statistically significant, confirming that mass shootings reduce local housing-market hotness.

Lagged temporary net migration is positively and statistically significantly associated with market hotness, while lagged permanent net migration is not statistically significant at conventional levels.

Crucially, neither the interaction between temporary net migration and mass shootings nor the interaction between permanent net migration and mass shootings is statistically significant, and both estimates are economically small. The average marginal effect of a mass shooting remains negative and statistically significant, and the joint test rejects that the mass-shooting indicator and interaction terms are jointly equal to zero.

These results indicate that while migration dynamics are correlated with baseline housing-market conditions, they do not materially condition the housing-market response to mass shootings. The negative effect of mass shootings on market hotness is similar across ZIP codes with varying degrees of short-term population churn and longer-run residential mobility. Taken together, the evidence suggests that mass shootings constitute a broadly salient local shock whose impact on housing demand is not mediated by migration patterns.

Insert Table 18

4.6.10. Behavioral Amplification: Mental Health

Mass shootings are salient, emotionally charged events that can affect economic behavior through behavioral channels, in addition to altering objective assessments of local risk. A large literature in behavioral economics and finance emphasizes that decision-making under uncertainty is shaped by risk aversion, limited attention, and affective responses. Individuals exhibit reference-dependent preferences and respond asymmetrically to negative shocks, consistent with loss aversion and heightened sensitivity to downside risk (Kahneman and Tversky, 1979; Rabin, 2000). When adverse events are vivid or emotionally salient, they attract disproportionate attention and are overweighted in belief formation, leading to distorted probability assessments and stronger behavioral responses (Tversky and Kahneman, 1973; Bordalo, Gennaioli, and Shleifer, 2012). A complementary strand shows that fear and psychological distress amplify perceived risk and accelerate belief updating beyond what is implied by objective information alone (Loewenstein et al., 2001; Lerner and Keltner, 2001). Consistent with this mechanism, exposure to negative experiences generates persistent shifts in risk beliefs and economic behavior (Malmendier and Nagel, 2011; Gennaioli and Shleifer, 2018).

Local mental health conditions provide a natural lens through which these mechanisms may operate. In areas with poorer baseline mental health, households may be more susceptible to salience-driven responses, overweighting the probability and consequences of extreme events. A mass shooting in such contexts may therefore trigger a sharper reassessment of neighborhood safety and a stronger pullback in housing demand.

If poorer mental health increases sensitivity to salient negative shocks and amplifies risk-averse behavior, the adverse effect of mass shootings on housing-market hotness should be more

pronounced in counties with worse baseline mental health. Accordingly, we hypothesize that poor mental health strengthens the negative housing-market response to mass shootings.

We test this hypothesis by extending our baseline ZIP–month fixed-effects specification to include county-level poor mental health days, measured as the average number of self-reported poor mental health days in the past 30 days and lagged by one year. We interact this measure with an indicator for mass shooting incidents. The dependent variable is the Market Hotness Index. All regressions include ZIP fixed effects, year–month fixed effects, and the full set of predetermined economic, demographic, and social controls described in Appendix A. Standard errors are clustered at the ZIP level.

The interaction coefficient identifies whether the housing-market response to mass shootings varies with pre-existing mental health conditions. The use of a one-year lag ensures that mental health is predetermined relative to the shooting event, alleviating concerns about reverse causality or contemporaneous psychological responses to the shooting itself.

Table 19 reports the results. The interaction between poor mental health days and the mass-shooting indicator is negative and statistically significant at the 1% level, indicating that mass shootings lead to a substantially larger decline in housing-market hotness in areas with poorer baseline mental health. In contrast, the main effect of poor mental health days is negative but statistically insignificant, and the standalone mass-shooting coefficient is economically small when evaluated at average mental health levels.

The average marginal effect of a mass shooting is negative and statistically significant, and the joint test of the mass-shooting indicator and its interaction strongly rejects the null of joint insignificance. These patterns are consistent with a salience-based and risk-aversion mechanism: mass shootings act as focal, emotionally charged shocks, and their housing-market impact is amplified where households are more vulnerable to fear-based belief updating.

Importantly, this mechanism differs from institutional or mobility-based channels. Unlike state gun laws or migration dynamics, mental health directly conditions households' psychological responses, suggesting that the observed heterogeneity reflects behavioral amplification rather than differences in objective exposure or sorting.

Taken together, these findings provide strong evidence that mental health is a key behavioral channel through which mass shootings affect housing markets. The housing-market response to mass shootings is significantly larger in areas with poorer baseline mental health, consistent with heightened risk aversion and salience-driven belief updating. Among the mechanisms we examine, this channel most directly captures the psychological transmission of violent shocks into economic behavior, highlighting the importance of mental well-being in shaping market responses to extreme events.

Insert Table 19

4.6.11. Credit-Supply Neutrality: Mortgage Credit Tightness

A natural concern in interpreting the decline in housing-market activity following mass shootings is that it may be driven by endogenous contractions in local mortgage credit supply, rather than by changes in households' willingness to buy or sell homes. In particular, lenders may respond to heightened local risk by tightening underwriting standards or withdrawing lending capacity, thereby mechanically depressing housing-market liquidity. If such lender pullbacks are the primary mechanism, the adverse effect of mass shootings should be more pronounced in areas where mortgage credit is already relatively tight.

To address this concern, we examine heterogeneity in the housing-market response to mass shootings across locations with differing levels of mortgage credit tightness. We proxy for local credit frictions using an indicator for above-national-average mortgage denial rates, defined as the ratio of the one-year lagged county-level mortgage denial rate to the national average denial rate, and equal to one when this ratio exceeds unity. Using a lagged measure ensures that local credit conditions are predetermined with respect to the mass shooting event, mitigating concerns that denial rates themselves adjust endogenously in response to contemporaneous shocks to local violence or housing-market activity.

This construction normalizes local mortgage-market conditions relative to the national lending environment, isolating persistent, location-specific differences in credit access from aggregate fluctuations in underwriting standards driven by nationwide business cycles, monetary policy, or regulatory changes. Interpreted economically, counties with above-national-average lagged denial rates represent environments in which households face greater ex ante difficulty accessing mortgage credit, either because lenders apply more stringent screening or because local borrower pools are more marginal. Housing transactions in these areas are therefore more likely to be constrained by credit supply even in normal times. If lender pullbacks drive the housing-market response to mass shootings, the negative effect of a shooting should be amplified in these high-denial locations.

We augment our baseline ZIP–month specification by interacting the mass shooting indicator with the one-year lagged above-national-average mortgage denial rate indicator. All regressions include ZIP fixed effects and year–month fixed effects, as well as the full set of predetermined economic, demographic, and social controls described in Appendix A. Standard errors are clustered at the ZIP level. The dependent variable remains the Market Hotness Index measured at the ZIP–month level.

Table 20 reports the results. Consistent with our baseline findings, the standalone effect of a mass shooting is negative, economically meaningful, and highly statistically significant, indicating a pronounced cooling of local housing-market conditions following such events. ZIP codes located in counties with above-national-average lagged mortgage denial rates exhibit higher baseline market hotness, reflecting tighter markets or stronger underlying demand prior to the shock.

Crucially, the interaction between mass shootings and the lagged above-national-average mortgage denial rate is positive and statistically significant, indicating that the adverse housing-market response to a mass shooting is attenuated in areas with tighter pre-existing mortgage credit conditions. Economically, this attenuation reflects the fact that housing markets in high-denial areas are already partially constrained by credit frictions, leaving less scope for an additional contraction in transaction activity following a local shock. In such environments, the pool of marginal buyers is smaller and turnover is already limited, so belief updating triggered by a mass shooting translates into a smaller incremental adjustment in observed market activity. This pattern is inconsistent with a lender-pullback mechanism, which would predict a stronger, not weaker, response in areas where credit supply is already tight and therefore more sensitive to adverse local shocks. The average marginal effect of a mass shooting remains negative and statistically significant, and a joint test strongly rejects the null that the coefficients on the mass shooting indicator and its interaction are jointly equal to zero.

By conditioning on predetermined local mortgage credit conditions, these results provide strong evidence against a lender-pullback explanation for the housing-market response to mass shootings. If endogenous contractions in mortgage credit supply were driving the observed decline in market activity, the negative effect of mass shootings should be strongest in locations with higher ex ante denial rates. Instead, we find the opposite pattern: areas with tighter pre-existing credit conditions experience a smaller incremental response to mass shootings. Overall, the evidence supports an interpretation in which mass shootings primarily affect housing markets through demand-side channels, such as belief updating about neighborhood safety and long-run residential desirability, rather than through contemporaneous shifts in mortgage credit supply.

Insert Table 20

5. Conclusion

This paper provides evidence that mass shootings weaken local housing-market conditions. Using a matched-control design with ZIP and time fixed effects, we show that markets exposed to a mass shooting experience a statistically and economically meaningful decline in Realtor.com's Market Hotness Index. Decomposing this response, we document a contraction in buyer interest, slower transactions, fewer new listings, and more frequent price concessions. Importantly, listing prices fall following an incident, indicating that reduced willingness to pay on the demand side dominates any offsetting effects from temporary supply withdrawal.

We further show that these effects are persistent rather than transitory and vary systematically with the informational content of incidents. Housing-market responses scale with the objective severity of mass shootings and are significantly amplified when public attention is elevated, consistent with an information-based belief-updating mechanism. By contrast, we find little evidence that housing-market responses operate through contemporaneous mortgage-credit contractions or population reallocation. Areas with tighter pre-existing credit conditions exhibit attenuated, rather than

amplified, responses, a pattern inconsistent with lender pullbacks and supportive of a demand-driven interpretation.

Our findings contribute to the literature linking crime and housing outcomes (Thaler, 1978; Gibbons, 2004; Cook and Ludwig, 2006) by showing that highly salient and extreme forms of gun violence impose immediate and persistent costs on local real estate markets. Beyond their tragic human toll, mass shootings reduce neighborhood desirability, erode household wealth embedded in housing, and weaken local economic resilience. By documenting these effects in housing—the largest asset held by most households—we highlight an important channel through which traumatic violence reshapes local economic outcomes.

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Appendix A: Variable Definitions and Data Sources

This Appendix entails the definitions of the variables and their data sources.

Name of variable	Definition of variable and data source
Mass shooting	Dummy (0,1) variable that is one when there is a mass shooting in a ZIP-code, else zero (Data source: Gun Violence Archive).
Total casualties of the mass shootings	Total casualties of the mass shootings (Data source: Gun Violence Archive).
Total victims killed in a mass shooting	Total victims killed in a mass shooting (Data source: Gun Violence Archive).
Total victims injured in a mass shooting	Total victims injured in a mass shooting (Data source: Gun Violence Archive).
Total suspects killed in a mass shooting	Total suspect killed in a mass shooting (Data source: Gun Violence Archive).
Total suspects injured in a mass shooting	Total suspect injured in a mass shooting (Data source: Gun Violence Archive).
Total suspects arrested in a mass shooting	Total suspects arrested in a mass shooting (Data source: Gun Violence Archive).
No suspect yet found in a mass shooting	Dummy (0,1) variable is 1 when the value of total suspects killed in a mass shooting, total suspects injured in a mass shooting, and total suspects arrested in a mass shooting is zero, else zero (Data source: Gun Violence Archive).
Market hotness index	An equally-weighted composite metric of a ZIP-code's supply score and demand score (Data source: realtor.com).
Demand score	An index representing a ZIP-code's unique listing page viewers per property ranking compared to other ZIP-codes.
Listing details page unique viewers per property ratio	The natural logarithm of the sum of one and the count of viewers a typical property receives in the specified ZIP-code divided by the count of views a typical property receives in the US overall during the same month (Data source: realtor.com).
Supply of real estate score	An index representing a ZIP-code's median days on market ranking compared to other ZIP-codes (Data source: realtor.com).
Median number of days property listings spend on the market	The natural logarithm of the sum of the one and the median number of days property listings spend on the market within the ZIP-code during the specified month. Time spent on the market is defined as the time between the initial listing of a property and either its closing date or the date it is taken off the market (Data source: realtor.com).
Median list price per square foot	The ratio of the median listing price within the specified ZIP-code during the specified month to the median square foot of a listing within the same ZIP-code during the specified month (Data source: realtor.com). We deflate the median listing price by using the GDP implicit price deflator with base year 2017 (i.e., the deflator is 100 if the year is 2017). The data source for the GDP implicit price deflator is Federal Reserve Bank of St. Louis.

New listings	The ratio of the count of new listings added to the market within the specified zip code to the count of total listings within the same ZIP code during the specified month. The new listing count represents a typical week's worth of new listings in a given month. The new listing count can be multiplied by the number of weeks in a month to produce a monthly new listing count. The total listing count tracks the number of for sale properties on the market, including pending listings where a pending status is available (Data source: realtor.com).
Share of listings with price reductions	The count of listings which have had their price reduced within the specified ZIP code to the count of total listings within the same ZIP code during the specified month. The price decrease count represents a typical week's worth of listings which have had their price reduced in a given month (Data source: realtor.com).
Social associations	Number of membership associations per 10,000 population (Data source: County Health Rankings Data).
Yearly population growth rate	Yearly population growth of a county (Data source: Bureau of Economic Analysis of the U.S. Department of Commerce).
Fraction of non-Hispanic African Americans	Percentage of non-Hispanic African American of a county (Data source: County Health Rankings Data).
Median household income	Natural logarithm of the median household income of a county (Data source: County Health Rankings Data). We deflate the median household income by using the GDP implicit price deflator with base year 2017 (i.e., the deflator is 100 if the year is 2017). The data source for the GDP implicit price deflator is Federal Reserve Bank of St. Louis.
Income inequality	Ratio of household income at the 80^{th} percentile to income at the 20^{th} percentile (Data source: County Health Rankings Data).
Violent crime rate	Number of reported violent crime offenses per 100,000 population (Data source: County Health Rankings Data).
Injury death rate	Number of deaths due to injury per 100,000 population (Data source: County Health Rankings Data).
COVID-19 period	Dummy (0,1) variable that is one if a county has COVID-19 deaths, else zero (Data source: New York Times).
Per capita personal income	Natural logarithm of the per capita personal income of a county (Data source: Bureau of Economic Analysis of the U.S. Department of Commerce). We deflate the per capita personal income by using the GDP implicit price deflator with base year 2017 (i.e., the deflator is 100 if the year is 2017). The data source for the GDP implicit price deflator is Federal Reserve Bank of St. Louis.

Yearly unemployment rate	Yearly unemployment rate of a county (Data source: U.S. Department of Labor, Bureau of Labor Statistics, Local Area Unemployment Statistics (LAUS)).
Yearly GDP growth rate	Yearly GPD growth of a county (Data source: Bureau of Economic Analysis of the U.S. Department of Commerce).
Access to firearm	The sum of federal firearms license type 01 (Dealer in firearms other than destructive devices (Includes gunsmiths) and type 09 (dealer in destructive devices) within the ZIP code by month (Data source: ATF Bureau of Alcohol, Tobacco, Firearms and Explosives)
Firm-arm regulatory leniency	A gun law strength within states index (Data source: Giffords Law Center).
Monthly housing return	Monthly housing return of a zip code by using Zillow Home Value Index (ZHVI) of All Homes (Data source: Zillow housing research data). We deflate the Zillow Home Value Index (ZHVI) of All Homes by using the GDP implicit price deflator with base year 2017 (i.e., the deflator is 100 if the year is 2017). The data source for the GDP implicit price deflator is Federal Reserve Bank of St. Louis.
Child Opportunity Index	The ratio of the average child opportunity index score of a county within their state to the average child opportunity index score of a county at national level (Data source: diversitydatakids.org).
State gun law weakness	Is the Gun law weakness rank (Data source: Giffords Law Center).
Republican oriented county	The ratio of votes for republican party presidential candidates to the total votes in the presidential candidates by county (Data source: MIT Election database).
Poor mental health days	Average number of mentally unhealthy days reported in past 30 days (age-adjusted) within a county (Data source: County Health Rankings & Roadmaps).
High public attention to mass shootings	An indicator that equals one if a state's monthly public interest in mass shootings exceeds the contemporaneous national average (i.e., the ratio is greater than one), and zero otherwise. The indicator therefore identifies months in which a state receives above-average public attention to mass shootings relative to other states, net of aggregate time variation in attention (Data source: Google Trends).
Temporary net migration zip	The difference between temporary change of address to zip and the temporary change of address from zip (Data source: USPS).
Permanent net migration zip	The difference between permanent change of address to zip and the permanent change of address from zip (Data source: USPS).
Mortgage denial rate	Number of denied rate to total mortgage applications (Data source: HMDA database).
Severity of the mass shooting index	The Mass Shooting Severity (MSS) index is a facts-based, continuous measure of exposure to mass shooting incidents at the ZIP–month level. The index is constructed from incident-

level data and captures three components: realized human harm, unresolved threat, and law-enforcement closure. Realized harm is defined as the logarithm of one plus a weighted sum of fatalities and injuries, $H_i = \log(1 + 2 \cdot \text{VictimsKilled}_i + \text{VictimsInjured}_i)$, where fatalities receive greater weight to reflect the discrete escalation in perceived severity associated with loss of life. Unresolved threat is measured by an indicator equal to one if no suspect is arrested, killed, or injured during the incident. Law-enforcement closure is measured as $C_i = \log(1 + \text{SuspectsArrested}_i + 0.5(2 \cdot \text{VictimsKilled}_i + \text{VictimsInjured}_i))$, capturing the extent to which the immediate threat is neutralized.

Incident-level severity is defined as $\text{MSS}_i^{raw} = H_i + \lambda_U U_i - \lambda_C C_i$, with weights $\lambda_U = 0.3$and $\lambda_C = 0.2$. The final MSS index is defined as $\text{MSS}_i = \max\{0, \text{MSS}_i^{raw}\}$, ensuring that ZIP–months without a mass shooting receive a value of zero. The ZIP–month MSS is obtained by summing MSS_iacross all incidents occurring within a ZIP code in a given month. Details on construction and aggregation are provided in Appendix B (Data source: Gun Violence Archive).

Appendix B: Construction of the Mass Shooting Severity (MSS) Index — Intuition and Numerical Example

A. What the MSS Index Measures

Our objective is to capture facts-based, incident-level severity of mass shootings in a way that is (i) monotone in realized harm, (ii) reflects residual uncertainty and threat when an incident is not immediately resolved, and (iii) allows aggregation to the ZIP–month level and accumulation over time. The MSS index is designed to operationalize the notion that mass shootings act as information shocks: households update beliefs not only based on casualties, but also based on whether the event leaves an unresolved threat and whether the incident ends with law-enforcement closure.

Accordingly, MSS incorporates three components:

1. Realized harm: fatalities and injuries (verifiable outcomes).
2. Unresolved threat: an indicator for whether the perpetrator is at large or the outcome is unknown, capturing heightened uncertainty and perceived ongoing risk.
3. Closure: an indicator for whether the incident ends through law-enforcement action (apprehension or neutralization), capturing resolution that plausibly reduces perceived ongoing risk relative to incidents without law-enforcement closure (e.g., suicide).

The index is constructed to equal zero in ZIP–months with no mass shooting exposure and to increase with both the severity and frequency of incidents in a given ZIP–month.

B. Step-by-Step Calculation (with Numerical Example)

B.1. Event-Level Inputs

For each incident e, we record:

- F_e: number of fatalities
- I_e : number of injuries
- Event outcome (used to construct threat and closure indicators)

We define two indicator variables:

- Unresolved threat: U_e=1{perpetrator at large or outcome unknown}
- Law-enforcement closure: C_e = 1{apprehended or neutralized by law enforcement}

B.2. Realized Harm Component

We first compute a realized-harm score:

$$H_e = F_e + \lambda\ I_e$$

where $\lambda \in (0,1)$ weights injuries relative to fatalities. In the numerical example below, we set $\lambda = 0.5$, so that two injuries are weighted equivalently to one fatality.

B.3. Threat and Closure Adjustment

We then adjust severity based on unresolved threat and closure status using a multiplicative factor:

$$T_e = 1 + \alpha U_e - \beta C_e$$

where $\alpha \geq 0$ captures the severity premium associated with unresolved threat and $\beta \geq 0$ captures the reduction associated with law-enforcement closure. In the example below, we set $\alpha = 0.25$ and $\beta = 0.10$.

B.4. Incident Severity

Event-level severity is:

$$S_e = H_e \times T_e$$

B.5. ZIP–Month Aggregation (Flow Exposure)

For ZIP z in month t, let $E(z,t)$ denote the set of incidents in that ZIP–month. The ZIP–month MSS exposure is:

$$MSS_{z,t}^{flow} = \sum_{e \epsilon E(z,t)} S_e$$

If no incidents occur in ZIP z during month t, then E(z,t) and $MSS_{z,t}^{flow} = 0$

B.6. Optional: Cumulative Exposure (Stock)

To capture persistence in information and beliefs, we optionally construct a cumulative measure:

$$MSS_{z,t}^{stock} = MSS_{z,t}^{flow} + \rho \; MSS_{z,t-1}^{flow}$$

where $\rho \in [0,1)$ governs decay. Higher ρ implies more persistent exposure.

B.7. Numerical Example

Consider ZIP 07030 in March 2021, with two incidents in the same month. Parameter choices are $\lambda = 0.5$, $\alpha = 0.25$, and $\beta = 0.10$.

Incident 1: Apprehended (Law-Enforcement Closure)

- Fatalities: $F_1 = 2$
- Injuries: $I_1 = 6$
- Outcome: apprehended $\Rightarrow U_1 = 0, C_1 = 1$

Step 1: Realized harm

$$H_1=2+0.5(6)=2+3=5$$

Step 2: Threat factor

$$T_1= 1 + 0.25(0) – 0.10(1) = 0.90$$

Step 3: Incident Severity

$$S_1= 5 \text{ x } 0.90 = 4.50$$

Incident 2: At Large (Unresolved Threat)

- Fatalities: $F_2 = 1$
- Injuries: $I_2 = 2$
- Outcome: at large ⇒ $U_2 = 1, C_2 = 0$

Step 1: Realized harm

$$H_2=1+0.5(2)=1+1=2$$

Step 2: Threat factor

$$T_2= 1 + 0.25(1) – 0.10(0) = 1.25$$

Step 3: Incident Severity

$$S_2= 2 \text{ x } 1.25 = 2.50$$

ZIP–Month MSS (Flow)

Aggregate incident severities within the ZIP–month:

$$MSS^{flow}_{07030,2021m03} = 4.50 + 2.50 = 7.00$$

Thus, ZIP 07030's MSS exposure in March 2021 equals 7.00. By construction, ZIP–months without an incident have MSS equal to 0, and months with multiple incidents accumulate exposure additively.

Optional: ZIP–Month MSS (Stock)

Suppose the cumulative exposure in February 2021 is $MSS^{flow}_{07030,2021m02} = 5.00$ and we set ρ = 0.80. Then:

$$MSS^{flow}_{07030,2021m03} = 7.00 + 0.80(5.00) = 11.00$$

This stock measure captures persistence in informational exposure, allowing past incidents to continue affecting belief formation and market outcomes.

Figure 1: Covariate Imbalance Graphing

Figure 1 visualizes covariate balance before and after 3-nearest neighbors matching. The figure plots the standardized percentage bias for each covariate in the unmatched sample (●) and the matched sample (×). Following Rosenbaum and Rubin (1985), standardized percentage bias is computed as the difference in sample means between treated and control groups divided by the square root of the average of the sample variances in both groups, expressed as a percentage. A reduction in bias after matching indicates improved covariate balance. Variable definitions are provided in Appendix A.

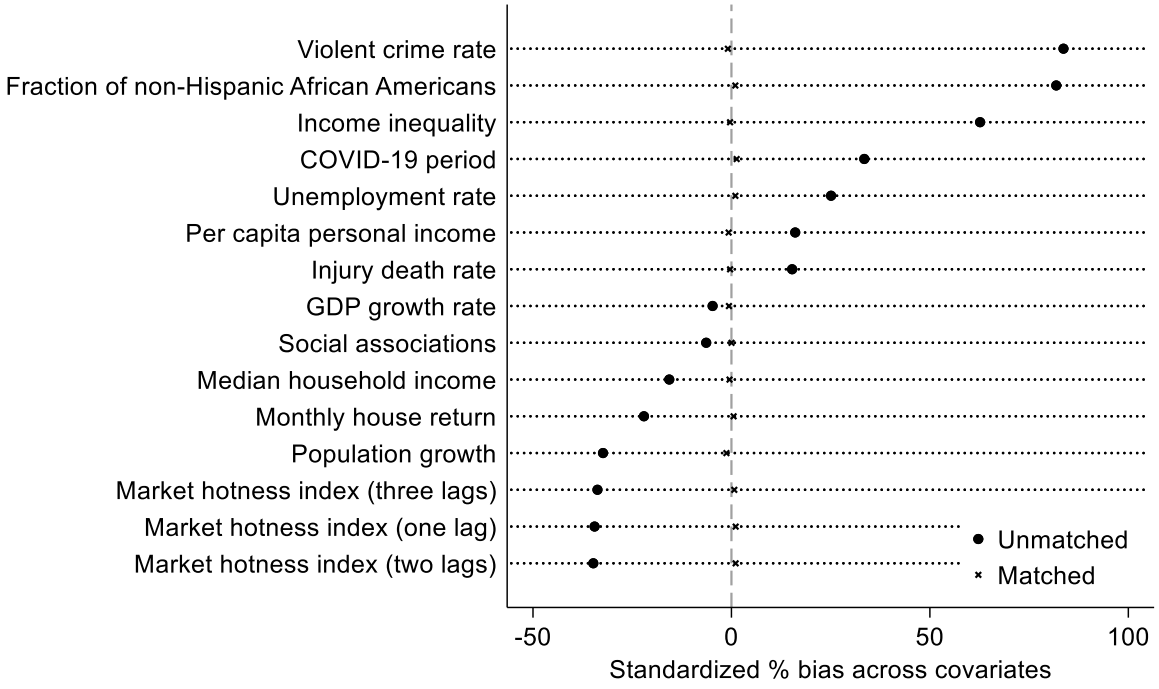
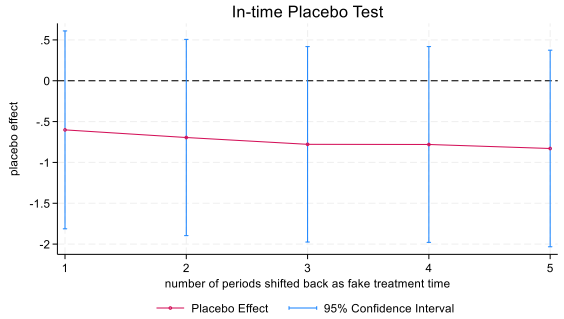


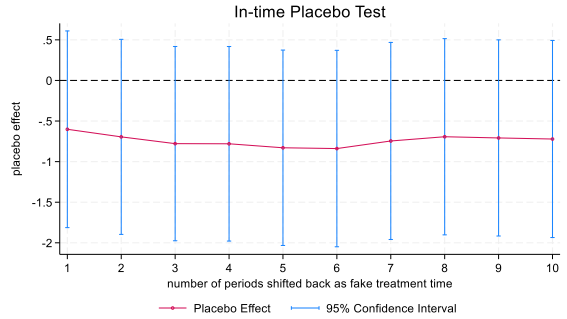
Figure 2: In-time Placebo Test

Figure 2 presents the results of the in-time placebo tests, which examine whether our estimates could be driven by pre-existing trends, slow-moving local shocks, or model artifacts. Specifically, we artificially shift the treatment date backward by 5 months (Panel A), 10 months (Panel B), 15 months (Panel C), and 20 months (Panel D) to generate placebo treatment assignments. Each panel plots the estimated placebo effects and the corresponding 95% confidence intervals across the shifted periods. If the estimated housing-market response were driven by spurious dynamics rather than the mass-shooting events themselves, we would expect to observe significant placebo effects even under these fictitious treatment timings. Instead, across all panels, the placebo estimates are statistically indistinguishable from zero and exhibit no systematic pattern. This evidence indicates the absence of differential pre-trends or spurious pre-event dynamics prior to the actual mass-shooting incidents. The results support a causal interpretation of the post-event decline in housing-market hotness and validate our inference regarding the persistence of the effect. Variable definitions are provided in Appendix A.

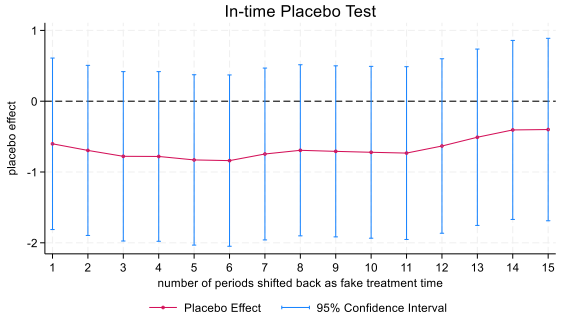
Panel A: In-Time Placebo Test (shifting 5 months back)



Panel B: In-Time Placebo Test (shifting 10 months back)



Panel C: In-Time Placebo Test (shifting 15 months back)



Panel D: In-Time Placebo Test (shifting 20 months back)

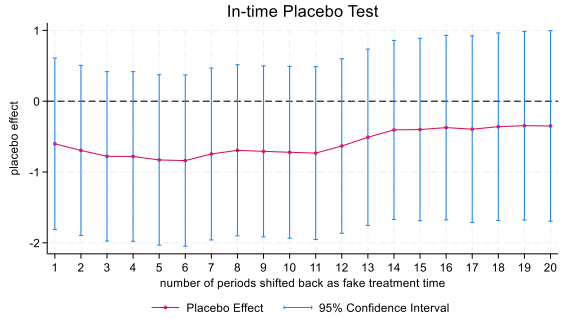


Figure 3: In-space Placebo Test Distribution

Figure 3 presents the results of the in-space placebo test, which assesses whether the estimated treatment effect could arise by chance due to spatial heterogeneity or random assignment rather than the occurrence of a mass shooting. Specifically, we construct the distribution of placebo effects obtained from 1,000 random reassignments of the treatment to ZIP codes that did not experience a mass shooting. The histogram displays the frequency distribution of these placebo estimates, while the blue curve overlays the corresponding kernel density estimate. The dashed vertical line marks zero, and the solid vertical line denotes the actual estimated treatment effect (−1.13). The true estimate lies deep in the left tail of the placebo distribution, indicating that the observed decline in housing-market hotness is highly unlikely to be generated by random spatial reassignment. This result provides strong evidence that our findings are not driven by chance spatial correlations or unobserved location-specific shocks, further supporting a causal interpretation of the estimated mass-shooting effects and reinforcing the validity of our persistence analysis.

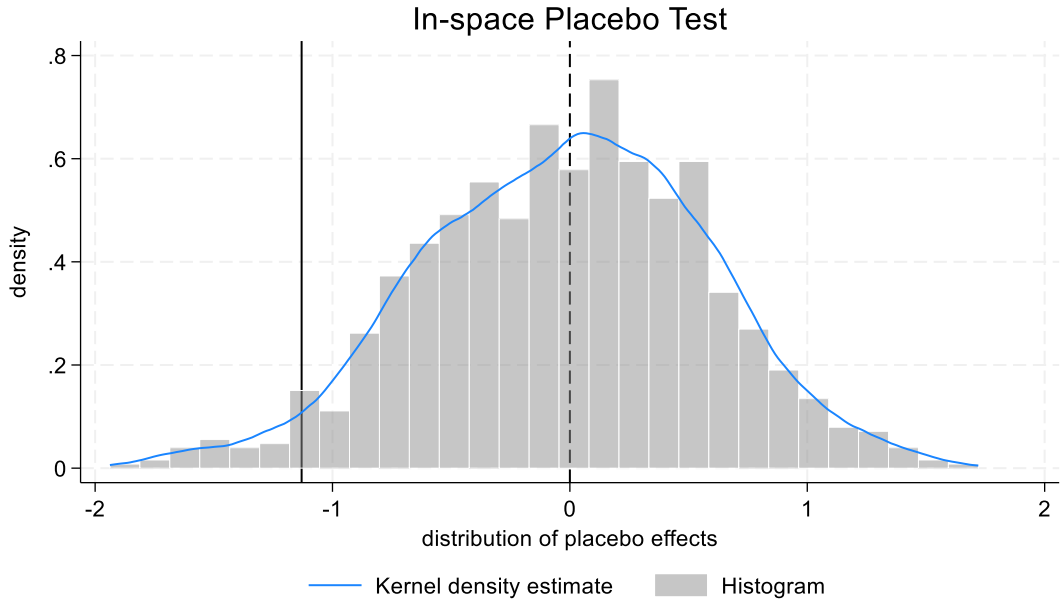


Table 1: Covariate imbalance testing following 3-neaurest neighbors matching

Table 1 reports the covariate balance before and after 3-nearest neighbors matching. A month-year ZIP code is classified as treated if it experienced a mass shooting and as control otherwise. The unmatched sample comprises the observations used for the matching procedure, while the matched sample consists of the observations retained after matching. The %bias column presents the standardized percentage bias between treated and control observations in both the unmatched and matched samples. Following Rosenbaum and Rubin (1985), standardized bias is calculated as the difference in sample means between treated and control groups divided by the square root of the average of the sample variances in both groups, expressed as a percentage. The % reduction of |bias| column reports the percentage decrease in absolute standardized bias attributable to matching, providing a measure of the improvement in covariate balance; values close to 100% indicate near-complete elimination of differences across groups. We also report the t-statistic and corresponding p-value from two-sample t-tests for equality of means, based on both the unmatched and matched samples. The final column presents the variance ratio (treated over control) for continuous covariates. A variance ratio of one in the matched sample indicates perfect covariate balance. All variable definitions are provided in Appendix A.

	Sample	Mean		%bias	%reduction of	t-test		V(T)/V(C)
		Treated	Control		|bias|	t	p>|t|	
Market hotness index (one lag)	Unmatched	41.70	49.67	-34.50	96.90	-13.41	0.00	0.94
	Matched	41.70	41.45	1.10		0.30	0.76	0.99
Market hotness index (two lags)	Unmatched	41.82	49.86	-34.80	97.00	-13.52	0.00	0.94
	Matched	41.82	41.58	1.10		0.30	0.77	1.00
Market hotness index (three lags)	Unmatched	42.34	50.15	-33.70	97.90	-13.11	0.00	0.94
	Matched	42.34	42.18	0.70		0.20	0.84	0.98
Social associations of a county	Unmatched	9.44	9.68	-6.40	99.80	-2.46	0.01	0.93
	Matched	9.44	9.44	0.00		0.00	1.00	0.91
Yearly population growth rate of a county	Unmatched	0.19	0.55	-32.30	96.10	-12.51	0.00	0.92
	Matched	0.19	0.21	-1.30		-0.34	0.73	0.81
Percentage of Non-Hispanic African Americans of a county	Unmatched	23.36	11.36	81.80	98.80	39.09	0.00	1.94
	Matched	23.36	23.22	1.00		0.23	0.82	0.91
Median household income of a county	Unmatched	10.90	10.94	-15.70	96.90	-6.09	0.00	0.94
	Matched	10.90	10.90	-0.50		-0.14	0.89	0.97
Income inequality of a county	Unmatched	5.12	4.64	62.70	99.40	28.52	0.00	1.67
	Matched	5.12	5.12	-0.40		-0.09	0.93	0.95

Violent crime rate of a county	Unmatched	578.41	351.40	83.70	98.90	40.18	0.00	1.97
	Matched	578.41	580.87	-0.90		-0.22	0.83	0.96
Injury death rate of a county	Unmatched	75.62	72.11	15.30	97.80	6.30	0.00	1.18
	Matched	75.62	75.70	-0.30		-0.09	0.93	0.90
COVID-19 period	Unmatched	0.53	0.37	33.50	96.10	13.43	0.00	n.a.
	Matched	0.53	0.53	1.30		0.36	0.72	n.a.
Per capita personal income of a county	Unmatched	10.87	10.82	16.10	95.50	6.28	0.00	0.96
	Matched	10.87	10.87	-0.70		-0.20	0.85	0.89
Yearly unemployment rate of a county	Unmatched	5.14	4.64	25.10	96.10	10.81	0.00	1.38
	Matched	5.14	5.12	1.00		0.26	0.80	1.03
Yearly GDP growth rate of a county	Unmatched	2.04	2.23	-4.70	86.60	-1.88	0.06	1.02
	Matched	2.04	2.07	-0.60		-0.18	0.86	1.02
Monthly housing return of a zip code	Unmatched	0.03	0.04	-22.00	97.60	-9.56	0.00	1.42
	Matched	0.03	0.03	-0.50		0.14	0.89	1.11

Table 2: Summary Statistics of the Full Sample

We present the summary statistics of the full sample. Variable definitions are in Appendix A of the paper.

Variables	Obs.	Mean	Stdev.	Min	P50	Max
Mass shooting	41,838	0.04	0.20	0.00	0.00	1.00
Market hotness index	41,838	48.71	22.09	3.65	48.76	95.99
Demand score	41,838	47.09	26.30	1.12	45.58	98.88
Listing details page unique viewers per property	41,838	0.66	0.23	0.18	0.64	1.37
Supply score	41,838	50.33	27.13	1.17	49.60	98.66
Median number of days property listings spend on the market within the zip code	41,838	3.96	0.44	2.77	3.97	5.11
Share of listings with reduced sale price	41,838	0.17	0.10	0.00	0.16	0.51
Median list price per square foot	41,838	183.92	145.83	43.00	136.67	952.12
New listings	41,838	0.33	0.16	0.00	0.30	0.86
Social associations	41,838	9.29	3.54	3.37	8.75	22.45
Yearly population growth rate	41,838	0.43	1.17	-2.20	0.30	4.10
Percentage of Non-Hispanic African Americans	41,838	16.39	14.64	0.34	11.20	57.24
Median household income	41,838	10.90	0.26	10.41	10.88	11.57
Income inequality	41,838	4.76	0.68	3.44	4.70	7.10
Violent crime rate	41,838	401.58	219.32	43.42	373.19	1191.94
Injury death rate	41,838	71.03	22.06	33.11	69.00	137.38
COVID-19 period	41,838	0.43	0.49	0.00	0.00	1.00
Per capita personal income	41,838	10.78	0.25	10.32	10.75	11.59
Unemployment rate	41,838	4.96	1.90	2.20	4.50	11.50
GDP growth rate	41,838	2.06	4.05	-9.40	2.40	13.50
Monthly housing return	41,838	0.04	0.06	-0.10	0.04	0.23

Table 3: Summary Statistics of the Mass Shootings

We present the summary statistics of the mass shootings in our sub-sample of mass shootings. The sub-sample of mass shootings only includes ZIP codes with a mass shooting. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are in Appendix A of the paper.

Variables	Obs.	Mean	Stdev.	Min	P50	Max
Total casualties of the mass shootings	1,702	5.88	3.38	4.00	5.00	23.00
Total victims killed in a mass shooting	1,702	1.13	1.42	0.00	1.00	7.00
Total victims injured in a mass shooting	1,702	4.72	3.06	0.00	4.00	18.00
Total suspects killed in a mass shooting	1,702	0.06	0.24	0.00	0.00	1.00
Total suspects injured in a mass shooting	1,702	0.05	0.21	0.00	0.00	1.00
Total suspects arrested in a mass shooting	1,702	0.70	1.07	0.00	0.00	5.00
No suspect yet found in a mass shooting	1,702	0.51	0.50	0.00	1.00	1.00

Table 4: Summary Statistics of the Mass Shootings by state

This table reports state-level summary statistics for mass-shooting incidents in our sample period. For each state, we list the number of incidents and the incident-level means of total casualties, victims killed, victims injured, suspects killed, suspects injured, suspects arrested, and the mean share of incidents with no suspect identified. All statistics are computed over incidents observed within a state.

State	Number of mass shootings	Mean total casualties	Mean total victims killed	Mean total victims injured	Mean total suspect killed	Mean total suspect injured	Mean total suspect arrested	Mean percentage of no suspect yet found in a mass shooting
AL	28.00	5.61	1.00	4.57	0.07	0.07	0.86	0.39
AR	28.00	5.25	0.92	4.33	0.00	0.13	0.71	0.42
AZ	33.00	6.18	1.29	4.89	0.11	0.04	0.64	0.46
CA	191.00	5.42	1.20	4.15	0.08	0.02	0.59	0.56
CO	50.00	5.30	1.30	3.94	0.04	0.15	0.81	0.47
DC	58.00	7.66	1.05	6.50	0.03	0.03	0.66	0.61
DE	14.00	5.08	0.75	4.33	0.08	0.00	0.17	0.83
FL	113.00	5.54	1.07	4.44	0.07	0.05	0.70	0.51
GA	107.00	5.32	1.07	4.23	0.07	0.03	0.68	0.52
HI	1.00	5.00	2.00	3.00	0.00	0.00	2.00	0.00
IA	6.00	5.00	0.83	4.17	0.00	0.00	1.17	0.50
ID	3.00	4.33	2.33	2.00	0.67	0.00	0.33	0.00
IL	64.00	5.68	1.21	4.44	0.08	0.03	0.52	0.60
IN	44.00	6.08	1.29	4.76	0.03	0.11	0.55	0.58
KS	2.00	5.50	1.50	4.00	0.00	0.00	1.00	0.00
KY	33.00	5.45	0.65	4.81	0.00	0.03	0.87	0.55
LA	119.00	6.17	0.98	5.17	0.04	0.07	0.86	0.39
MA	16.00	5.29	0.57	4.71	0.07	0.07	0.71	0.43
MD	96.00	6.46	1.04	5.41	0.06	0.03	0.63	0.61
ME	1.00	4.00	3.00	1.00	0.00	0.00	1.00	0.00
MI	73.00	6.00	1.26	4.71	0.03	0.07	0.34	0.66
MN	45.00	6.88	0.73	6.03	0.00	0.09	0.79	0.58
MO	61.00	6.04	1.42	4.62	0.02	0.09	0.67	0.53

MS	21.00	4.81	1.38	3.43	0.05	0.00	1.29	0.29
MT	2.00	4.00	2.00	2.00	0.50	0.00	0.50	0.00
NC	91.00	4.85	1.04	3.81	0.04	0.04	0.60	0.54
ND	1.00	4.00	1.00	3.00	1.00	0.00	0.00	0.00
NE	5.00	6.50	0.00	6.50	0.00	0.00	0.75	0.75
NJ	25.00	5.48	0.52	4.96	0.00	0.00	0.83	0.65
NM	6.00	5.50	1.50	4.00	0.33	0.00	1.00	0.00
NV	13.00	7.70	2.00	5.70	0.20	0.10	1.40	0.10
NY	64.00	5.87	0.58	5.27	0.02	0.00	0.67	0.60
OH	60.00	5.51	1.06	4.47	0.08	0.08	0.42	0.62
OK	3.00	6.00	4.33	1.67	0.33	0.00	0.33	0.33
OR	16.00	5.57	0.79	4.79	0.07	0.07	0.21	0.71
PA	156.00	8.68	1.37	7.15	0.07	0.06	0.77	0.56
RI	2.00	4.50	0.00	4.50	0.00	0.50	2.50	0.50
SC	69.00	5.42	1.01	4.40	0.06	0.00	1.21	0.34
TN	73.00	6.03	1.08	4.95	0.07	0.07	0.93	0.44
TX	163.00	6.09	1.48	4.60	0.11	0.02	0.60	0.52
UT	2.00	5.00	2.00	3.00	0.00	0.50	3.00	0.00
VA	7.00	6.14	1.43	4.00	0.14	0.00	0.29	0.57
WA	25.00	5.04	1.00	4.04	0.04	0.08	0.67	0.54
WI	17.00	6.14	1.07	5.07	0.21	0.00	1.36	0.00
WV	1.00	7.00	0.00	7.00	0.00	0.00	1.00	0.00
WY	1.00	4.00	2.00	2.00	0.00	0.00	1.00	0.00

Table 5 — Baseline Effect of Mass Shootings on Market Hotness

This table reports estimates from the baseline specification. The dependent variable is the Market Hotness Index at the ZIP–month level. All regressions include ZIP and year–month fixed effects and the predetermined economic, demographic, and social controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered by ZIP. t-statistics are shown in parentheses beneath coefficients. For brevity, coefficients (and t-statistics) on the fixed effects are omitted. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * for the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index (1)
Mass shooting	-1.1301***
	(-3.22)
Social associations	-1.6143***
	(-3.12)
Population growth	2.3804***
	(6.11)
Fraction of Non-Hispanic African Americans	-0.7659
	(-1.13)
Median household income	-23.1510***
	(-4.05)
Income inequality	2.5646*
	(1.81)
Violent crime rate	-0.0126**
	(-2.54)
Injury death rate	0.2061***
	(4.22)
COVID-19 period	-1.2525***
	(-2.72)
Per capita personal income	2.8486
	(0.29)
Unemployment rate	-3.0340***
	(-11.28)
GDP growth rate	0.0404
	(0.72)
Monthly housing return	65.9141***
	(14.32)
Constant	287.7209***
	(2.60)
ZIP FE	Yes
Month-year FE	Yes
Observations	41,838

Adj. R2	0.66

Table 6 — Demand-Side Effects of Mass Shootings

This table reports estimates from the baseline specification with two alternative dependent variables that proxy buyer interest at the ZIP–month level: (i) Demand Score (a relative index of unique listing-page viewers per property) and (ii) the viewers-per-property ratio (unique listing-page viewers per property in the ZIP normalized by the national benchmark). All regressions include ZIP and year–month fixed effects and the predetermined controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered by ZIP; t-statistics appear in parentheses beneath coefficients. For brevity, coefficients and t-statistics on controls and fixed effects are omitted. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * for the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Demand score	Listing details page unique viewers per property ratio
Mass shooting	-1.0945*** (-3.05)	-0.0101*** (-3.33)
Intercept and control variables	Yes	Yes
ZIP FE	Yes	Yes
Month-year FE	Yes	Yes
Observations	41,838	41,838
Adj. R2	0.74	0.75

Table 7 — Supply-Side Effects of Mass Shootings

This table reports estimates from the baseline specification with four alternative dependent variables capturing seller behavior and market tightness at the ZIP–month level: (i) Supply Score (relative index based on days on market), (ii) Median Days on Market, (iii) New Listings (share of newly listed properties), and (iv) Share of Listings with Price Reductions. All regressions include ZIP and year–month fixed effects and the predetermined controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered by ZIP; t-statistics are shown in parentheses beneath coefficients. For brevity, coefficients (and t-statistics) on controls and fixed effects are omitted. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * for the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Supply Score	Median Days on Market	New Listings	Share of Listings with Price Reductions
	(1)	(2)	(3)	(4)
Mass shooting	-1.1447** (-2.29)	0.0158** (2.30)	-0.0069** (-2.34)	0.0043** (2.03)
Intercept and control variables	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes
Observations	41,838	41,838	41,838	41,838
Adj. R2	0.57	0.65	0.58	0.47

Table 8 — Price Effects: Listing Price per Square Foot

This table reports estimates from the baseline specification with the dependent variable set to the median listing price per square foot (inflation-adjusted as described in Appendix A). All regressions include ZIP and year–month fixed effects and the predetermined controls detailed in Appendix A. Heteroskedasticity-robust standard errors are clustered by ZIP; t-statistics appear in parentheses beneath coefficients. For brevity, coefficients (and t-statistics) on controls and fixed effects are omitted. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * for the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Median listing price per square foot (1)
Mass shooting	-2.0687**
	(-2.14)
Intercept and control variables	Yes
ZIP FE	Yes
Month-year FE	Yes
Observations	41,838
Adj. R2	0.98

Table 9 — Robustness: Alternative Numbers of Matched Controls

This table reports baseline estimates using samples constructed via nearest-neighbor propensity-score matching that vary the number of matched controls per treated ZIP from 1 to 10. The dependent variable is the Market Hotness Index at the ZIP–month level. All regressions include ZIP and year–month fixed effects and the predetermined controls described in Appendix A. Standard errors are heteroskedasticity-robust and clustered by ZIP; t-statistics appear in parentheses beneath coefficients. For brevity, coefficients (and t-statistics) on controls and fixed effects are omitted. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * for the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index				
	(1)	(2)	(3)	(4)	(5)
#matches	1	2	3	4	5
Mass shooting	-1.1265*** (-3.21)	-1.1280*** (-3.22)	-1.1301*** (-3.22)	-1.1480*** (-3.27)	-1.1553*** (-3.29)
Intercept and control variables	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
Observations	41,453	41,632	41,838	42,123	42,282
Adj. R2	0.66	0.66	0.66	0.66	0.66

Continued

Dependent variable:	Market hotness index				
	(6)	(7)	(8)	(9)	(10)
#matches	6	7	8	9	10
Mass shooting	-1.1552***	-1.1606***	-1.1600***	-1.1660***	-1.1669***
	(-3.29)	(-3.30)	(-3.30)	(-3.32)	(-3.32)
Intercept and control variables	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
Observations	42,393	42,655	42,733	42,933	42,963
Adj. R2	0.66	0.66	0.66	0.66	0.66

Table 10 — Baseline Effect on Market Hotness: Excluding the COVID-19 Period

This table reports estimates from the baseline specification on a sample that excludes the COVID-19 period (2020–2021); the COVID indicator is therefore omitted. The dependent variable is the Market Hotness Index at the ZIP–month level. All regressions include ZIP and year–month fixed effects and the predetermined controls described in Appendix A. Standard errors are heteroskedasticity-robust and clustered by ZIP; t-statistics appear in parentheses beneath coefficients. For brevity, coefficients (and t-statistics) on controls and fixed effects are omitted. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * for the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index (1)
Mass shooting	-1.7341***
	(-3.38)
Intercept and control variables	Yes
ZIP FE	Yes
Month-year FE	Yes
Observations	24,032
Adj. R2	0.73

Table 11 — Additional Checks: Heterogeneity Tests of Local Conditions

This table reports estimates from the baseline specification re-estimated in two subsamples defined by local conditions. Panel A and Panel B present the first and second subsamples, respectively. The dependent variable is the Market Hotness Index at the ZIP–month level. All regressions include ZIP and year–month fixed effects and the predetermined controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered by ZIP; t-statistics are shown in parentheses beneath coefficients. For brevity, coefficients (and t-statistics) on controls and fixed effects are omitted. The final row reports the χ^2 statistic (and p-value) from a test of equality of the mass-shooting coefficient across the two subsamples; the null is equality. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * for the 1%, 5%, and 10% levels, respectively.

Dependent variable: Market hotness index					
Panel A: High risk of mass shooting incident					
	High unemployment rate	Low GDP growth rate	High income inequality	High violent crime rate	Low median household income
	(1)	(2)	(3)	(4)	(5)
Mass shooting	-0.7419	-1.2797**	-1.3672***	-1.0610***	-0.6195
	(-1.46)	(-2.49)	(-3.15)	(-2.60)	(-1.25)
Panel B: Low risk of mass shooting incident					
	Low unemployment rate	High GDP growth rate	Low income inequality	Low violent crime rate	High median household income
	(11)	(12)	(13)	(14)	(15)
Mass shooting	-1.0530**	-0.8364*	-0.5962	-0.8428	-1.4623***
	(-2.12)	(-1.75)	(-1.10)	(-1.32)	(-3.17)
Intercept and control variables	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
Observations	41,838	41,838	41,838	41,838	41,838

Coefficient of Mass shooting is different across Panels A and B (Chi-2 test statistic)	0.17	0.39	1.24	0.08	1.58

Continued

Dependent variable: Market hotness index					
Panel A: High risk of mass shooting incident					
	High injury death rate	More firm-arm regulatory leniency	Better access to firearms	Low fraction of African American	Low social association
	(6)	(7)	(8)	(9)	(10)
Mass shooting	-0.7829 (-1.60)	-0.5563 (-1.13)	-0.6810 (-1.09)	-1.3442*** (-2.63)	-1.3525*** (-2.83)
Panel B: Low risk of mass shooting incident					
	Low injury death rate	Less firm-arm regulatory leniency	Worser access to firearms	High fraction of African American	High social association
	(16)	(17)	(18)	(19)	(20)
Mass shooting	-1.3701*** (-3.09)	-1.7796*** (-3.72)	-0.8507* (-1.70)	-1.0057** (-2.18)	-1.4532*** (-3.05)
Intercept and control variables	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
Observations	41,838	41,761	29,526	41,838	41,838
Coefficient of Mass shooting is different across Panels A and B (Chi-2 test statistic)	0.77	3.56*	0.83	0.24	0.02

Table 12. Persistence of the Mass-Shooting Effect on Housing-Market Hotness

This table reports estimates from the dynamic specification in equation (2). The dependent variable is the Market Hotness Index measured at the ZIP–month level. Columns (1)–(6) report results from models that include two, four, six, eight, ten, and twelve lags of the mass-shooting indicator, respectively. All regressions include ZIP-code and year–month fixed effects, as well as the predetermined economic, demographic, and social controls described in Appendix A. Standard errors are heteroskedasticity-robust and clustered at the ZIP-code level, with t-statistics reported in parentheses beneath coefficient estimates. For brevity, coefficients and t-statistics for the intercept, control variables, and fixed effects are omitted. The penultimate row reports the F-statistic from a joint test of whether the coefficients on the lagged mass-shooting indicators are jointly equal to zero. The final row reports the cumulative effect of mass shootings, expressed as a percentage of the mean and in standard deviation units of the Market Hotness Index. For example, the estimate in column (1) corresponds to a decline of approximately **3.5% of the sample mean**, or **0.08 standard deviations**, indicating an economically meaningful cooling of local housing-market conditions. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index					
≠lags	2 lags	4 lags	6 lags	8 lags	10 lags	12 lags
	(1)	(2)	(3)	(4)	(5)	(6)
Mass shooting$_{t-1}$	-0.9898***	-0.9946***	-1.0157***	-0.9632**	-1.0005**	-0.9784**
	(-2.80)	(-2.79)	(-2.74)	(-2.52)	(-2.57)	(-2.45)
Mass shooting $_{t-2}$	-0.7189**	-0.4809	-0.5591	-0.5233	-0.5122	-0.6739*
	(-2.01)	(-1.33)	(-1.48)	(-1.36)	(-1.28)	(-1.67)
Mass shooting $_{t-3}$		-0.7284**	-0.6698*	-0.8044**	-0.6631	-0.7468*
		(-1.97)	(-1.74)	(-1.99)	(-1.61)	(-1.77)
Mass shooting $_{t-4}$		-0.9702***	-1.0408***	-1.0890***	-1.2163***	-1.1467***
		(-2.68)	(-2.78)	(-2.83)	(-3.05)	(-2.74)
Mass shooting $_{t-5}$			-0.8908**	-0.9195**	-0.9306**	-0.8744*
			(-2.23)	(-2.25)	(-2.15)	(-1.93)
Mass shooting $_{t-6}$			-0.4774	-0.5287	-0.5840	-0.6579
			(-1.30)	(-1.36)	(-1.46)	(-1.54)

Mass shooting $_{t-7}$				-0.5281	-0.5521	-0.5505
				(-1.33)	(-1.37)	(-1.32)
Mass shooting $_{t-8}$				-0.3745	-0.3762	-0.4049
				(-0.91)	(-0.89)	(-0.92)
Mass shooting $_{t-9}$					0.0944	0.0460
					(0.21)	(0.10)
Mass shooting $_{t-10}$					-0.4346	-0.4924
					(1.01)	(-1.10)
Mass shooting $_{t-11}$						-0.4071
						(-0.87)
Mass shooting $_{t-12}$						-0.0479
						(-0.10)
Intercept	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	40,239	37,555	35,211	33,073	31,150	29,394
Adj. R2	0.68	0.67	0.67	0.67	0.68	0.68
Joint significance of interacted terms and interaction terms	-1.70***	-3.17***	-4.65***	-5.73**	-6.18**	-6.93*
Cumulative economic effect	-3.50% of mean -0.077 SD	-6.57% of mean -0.145 SD	-9.67% of mean -0.213 SD	-11.96% of mean -0.263 SD	-12.92% of mean -0.284 SD	-14.52% of mean -0.320 SD

Table 13: Robustness of the Treatment Definition: A Facts-Based Mass Shooting Severity Index

This table reports estimates examining the robustness of the effect of mass shooting incidents on housing-market conditions against the importance of the severity of the mass shooting. We use three variables to measure the effect of mass shootings: the number of victims of the mass shooting killed, the number of victims of the mass shooting injured and the number of arrested shooters of the mass shooting. The dependent variable is the Market Hotness Index measured at the ZIP–month level. The baseline specification includes the n the severity of the mass shooting instead of the mass shooting dummy variable. All regressions include ZIP fixed effects and year–month fixed effects, as well as the predetermined economic, demographic, and social controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered at the ZIP level, and *t*-statistics are reported in parentheses below coefficient estimates. For brevity, coefficients and *t*-statistics for the intercept, control variables, and fixed effects are omitted. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index
	(1)
Severity of the mass shooting index	-0.5749***
	(-3.50)
Intercept	Yes
Control variables	Yes
ZIP FE	Yes
Month-year FE	Yes
Observations	41,838
Adj. R2	0.66

Table 14. Moderating Effect of Public Attention to Mass Shootings on the Impact of Mass Shootings on Housing-Market Conditions

This table reports estimates examining whether public attention to mass shootings moderates the effect of mass shooting incidents on local housing-market conditions. The dependent variable is the Market Hotness Index measured at the ZIP–month level. The main specification includes high public attention to mass shootings and its interaction with mass shooting indicator. All regressions include ZIP and year–month fixed effects, as well as the predetermined economic, demographic, and social controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered at the ZIP level, and *t*-statistics are reported in parentheses below coefficient estimates. For brevity, coefficients and *t*-statistics for the intercept, control variables, and fixed effects are omitted. In the penultimate row, we report the average marginal effect of a mass shooting, evaluated at the sample means of the covariates. *t*-statistics for the marginal effects are constructed using delta-method standard errors. The final row reports the *F*-statistic for a joint test of whether the coefficients on the mass shooting indicator, the high public attention indicator, and their interaction are jointly different from zero. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index (1)
Mass shooting	-0.1729
	(-0.37)
High public attention to mass shootings	1.6167***
	(7.25)
High public attention to mass shootings x mass shooting	-1.9113***
	(-3.09)
Intercept	Yes
Control variables	Yes
ZIP FE	Yes
Month-year FE	Yes
Observations	41,838
Adj. R2	0.66
Average marginal effect of mass shooting	-1.1244***
	(-3.24)
Joint significance of interacted terms and interaction terms	22.47***

Table 15. Moderating Effect of Republican Oriented County to Mass Shootings on the Impact of Mass Shootings on Housing-Market Conditions

This table reports estimates examining whether republican orientation of a county moderates the effect of mass shooting incidents on local housing-market conditions. The dependent variable is the Market Hotness Index measured at the ZIP–month level. The main specification includes republican oriented county and its interaction with mass shooting indicator. All regressions include ZIP and year–month fixed effects, as well as the predetermined economic, demographic, and social controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered at the ZIP level, and *t*-statistics are reported in parentheses below coefficient estimates. For brevity, coefficients and *t*-statistics for the intercept, control variables, and fixed effects are omitted. In the penultimate row, we report the average marginal effect of a mass shooting, evaluated at the sample means of the covariates. *t*-statistics for the marginal effects are constructed using delta-method standard errors. The final row reports the *F*-statistic for a joint test of whether the coefficients on the mass shooting indicator, the republican oriented county, and their interaction are jointly different from zero. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index (1)
Mass shooting	-3.0644***
	(-4.12)
Republican oriented county	-9.1688***
	(-13.76)
Republican oriented county x mass shooting	5.2586***
	(2.97)
Intercept	Yes
Control variables	Yes
ZIP FE	Yes
Month-year FE	Yes
Observations	41,838
Adj. R2	0.66
Average marginal effect of mass shooting	-0.7775**
	(-2.14)
Joint significance of interacted terms and interaction terms	67.89***

Table 16. Moderating Effect of the Child Opportunity Index on the Impact of Mass Shootings on Housing-Market Conditions

This table reports estimates examining whether local child opportunity moderates the effect of mass shooting incidents on housing-market conditions. The dependent variable is the Market Hotness Index measured at the ZIP–month level. The baseline specification includes the Child Opportunity Index and its interaction with an indicator for mass shooting incidents. All regressions include ZIP fixed effects and year–month fixed effects, as well as the predetermined economic, demographic, and social controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered at the ZIP level, and *t*-statistics are reported in parentheses below coefficient estimates. For brevity, coefficients and *t*-statistics for the intercept, control variables, and fixed effects are omitted. The penultimate row reports the average marginal effect of a mass shooting, evaluated at the sample means of the covariates. *t*-statistics for the marginal effects are constructed using delta-method standard errors. The final row reports the *F*-statistic from a joint test of whether the coefficients on the mass shooting indicator and its interaction with the Child Opportunity Index are jointly equal to zero. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index
	(1)
Mass shooting	-5.2214***
	(-3.54)
Child Opportunity Index	13.2688***
	(5.37)
Child Opportunity Index x mass shooting	3.7915***
	(2.85)
Intercept	Yes
Control variables	Yes
ZIP FE	Yes
Month-year FE	Yes
Observations	41,838
Adj. R2	0.66
Average marginal effect of mass shooting	-1.0239***
	(-2.94)
Joint significance of interacted terms and interaction terms	15.80***

Table 17: Moderating effect of State Gun Law Weakness on the Impact of Mass Shootings on Housing-Market Conditions

This table reports estimates examining whether state gun law weakness moderates the effect of mass shooting incidents on housing-market conditions. The dependent variable is the Market Hotness Index measured at the ZIP–month level. The baseline specification includes the state gun law weakness and its interaction with an indicator for mass shooting incidents. All regressions include ZIP fixed effects and year–month fixed effects, as well as the predetermined economic, demographic, and social controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered at the ZIP level, and *t*-statistics are reported in parentheses below coefficient estimates. For brevity, coefficients and *t*-statistics for the intercept, control variables, and fixed effects are omitted. The penultimate row reports the average marginal effect of a mass shooting, evaluated at the sample means of the covariates. *t*-statistics for the marginal effects are constructed using delta-method standard errors. The final row reports the *F*-statistic from a joint test of whether the coefficients on the mass shooting indicator and its interaction with the state gun law weakness are jointly equal to zero. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index
	(1)
Mass shooting	-2.2151***
	(-3.80)
State gun law weakness	0.0884
	(0.98)
State gun law weakness x mass shooting	0.0532**
	(2.26)
Intercept	Yes
Control variables	Yes
ZIP FE	Yes
Month-year FE	Yes
Observations	41,761
Adj. R2	0.66
Average marginal effect of mass shooting	-1.0812***
	(-3.14)
Joint significance of interacted terms and interaction terms	5.72***

Table 18: Moderating effect of Net Migration Zip on the Impact of Mass Shootings on Housing-Market Conditions

This table reports estimates examining whether net migration zip moderates the effect of mass shooting incidents on housing-market conditions. The dependent variable is the Market Hotness Index measured at the ZIP–month level. The baseline specification includes the state gun law weakness and its interaction with an indicator for mass shooting incidents. All regressions include ZIP fixed effects and year–month fixed effects, as well as the predetermined economic, demographic, and social controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered at the ZIP level, and *t*-statistics are reported in parentheses below coefficient estimates. For brevity, coefficients and *t*-statistics for the intercept, control variables, and fixed effects are omitted. The penultimate row reports the average marginal effect of a mass shooting, evaluated at the sample means of the covariates. *t*-statistics for the marginal effects are constructed using delta-method standard errors. The final row reports the *F*-statistic from a joint test of whether the coefficients on the mass shooting indicator and its interaction with the net migration zip are jointly equal to zero. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index
	(1)
Mass shooting	-0.9703**
	(-2.03)
Temporary net migration zip	0.0549***
	(12.78)
Temporary net migration zip x mass shooting	-0.0027
	(-0.13)
Permanent net migration zip	0.0039
	(1.63)
Permanent net migration zip x mass shooting	-0.0067
	(-0.88)
Intercept	Yes
Control variables	Yes
ZIP FE	Yes
Month-year FE	Yes
Observations	26,226
Adj. R2	0.65
Average marginal effect of mass shooting	-0.8125*
	(-1.94)
Joint significance of interacted terms and interaction terms	37.01***

Table 19: Moderating effect of Poor Mental Health Days on the Impact of Mass Shootings on Housing-Market Conditions

This table reports estimates examining whether poor mental health days moderates the effect of mass shooting incidents on housing-market conditions. The dependent variable is the Market Hotness Index measured at the ZIP–month level. The baseline specification includes the state gun law weakness and its interaction with an indicator for mass shooting incidents. All regressions include ZIP fixed effects and year–month fixed effects, as well as the predetermined economic, demographic, and social controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered at the ZIP level, and *t*-statistics are reported in parentheses below coefficient estimates. For brevity, coefficients and *t*-statistics for the intercept, control variables, and fixed effects are omitted. The penultimate row reports the average marginal effect of a mass shooting, evaluated at the sample means of the covariates. *t*-statistics for the marginal effects are constructed using delta-method standard errors. The final row reports the *F*-statistic from a joint test of whether the coefficients on the mass shooting indicator and its interaction with the poor mental health days are jointly equal to zero. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index
	(1)
Mass shooting	0.0426
	(0.08)
Poor mental health days	-0.0725
	(-1.33)
Poor mental health days x mass shooting	-0.1753***
	(-2.91)
Intercept	Yes
Control variables	Yes
ZIP FE	Yes
Month-year FE	Yes
Observations	41,838
Adj. R2	0.66
Average marginal effect of mass shooting	-1.0603***
	(-3.05)
Joint significance of interacted terms and interaction terms	7.13***

Table 20: Moderating effect of Mortgage Denial rate on the Impact of Mass Shootings on Housing-Market Conditions

This table reports estimates examining whether mortgage denial rate moderates the effect of mass shooting incidents on housing-market conditions. The dependent variable is the Market Hotness Index measured at the ZIP–month level. The baseline specification includes the state gun law weakness and its interaction with an indicator for mass shooting incidents. All regressions include ZIP fixed effects and year–month fixed effects, as well as the predetermined economic, demographic, and social controls described in Appendix A. Heteroskedasticity-robust standard errors are clustered at the ZIP level, and *t*-statistics are reported in parentheses below coefficient estimates. For brevity, coefficients and *t*-statistics for the intercept, control variables, and fixed effects are omitted. The penultimate row reports the average marginal effect of a mass shooting, evaluated at the sample means of the covariates. *t*-statistics for the marginal effects are constructed using delta-method standard errors. The final row reports the *F*-statistic from a joint test of whether the coefficients on the mass shooting indicator and its interaction with mortgage denial rate are jointly equal to zero. Variable definitions and data sources are provided in Appendix A. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Market hotness index
	(1)
Mass shooting	2.0029**
	(2.20)
Mortgage denial rate	-55.6331***
	(-5.43)
Mortgage denial rate x mass shooting	-3.5945***
	(-3.73)
Intercept	Yes
Control variables	Yes
ZIP FE	Yes
Month-year FE	Yes
Observations	41,838
Adj. R2	0.66
Average marginal effect of mass shooting	-0.8141**
	(-2.28)
Joint significance of interacted terms and interaction terms	18.05***